

The Case Against Hysteria

BRITAIN is being assaulted by the environmentalists. This weekend, Dr Paul Ehrlich, president of Zero Population Growth Inc., and a professor of biology at Stanford University, is to recite for the Conservation Society his now familiar dirge that the world is about to breed itself to death. Last week, a distinguished group of doctors, many of whom should have known better, published in *The Lancet* and the *British Medical Journal* a declaration that Britain is so overcrowded that there is "a direct threat to the mental and physical well-being of our patients" and a plea that doctors should unite "to combat the British disease of over-population". At the same time, the new magazine *The Ecologist* published what it called "A Blueprint for Survival" which reflects and sometimes amplifies a good many of the half-baked anxieties about what is called the environmental crisis. On this occasion, the doctrine that dog should not eat dog notwithstanding, the magazine deserves to be taken to task if only for having recruited a "statement of support" from 33 distinguished people, many of them scientists, at least half of whom should have known better (see box). Nobody pretends that there are no serious problems to be worried about but the time seems fast approaching when the cry of disaster round the corner will have to be promoted to the top of the list of causes for public concern.

That professional people should lend their names to attempts like these to fan public anxiety about problems which have either been exaggerated or which are non-existent is reprehensible. It is especially regrettable that declarations like these should myopically draw attention to the supposed difficulties of moderating population growth in Britain when there is no evidence worth speaking of to suggest that Britain is over-populated (which is not, of course, the same thing as to say that the country is properly managed). The doctors who signed the round robin to the medical weeklies say that the problems of the developing countries "are formidable and may defy any rational solution", but that they are also "gravely concerned" at the pace of growth of the British population, which exceeds 55 million, and which is expected to increase to 66.5 million by the end of the century.

In reality, the doctors seem to have added an extra 500,000 to the latest estimates of the population of the United Kingdom in the year 2000, for the Government Actuary's latest calculation, published three months ago, gives an even 66 million for that date. It is, however, much more relevant that the forward projections of the British population have been declining steadily over the past decade, as the statisticians have been persuaded by experience that the trend of fertility in Britain, like that in much of the rest of Western Europe, is downward. The doctors also choose, by design or ignorance, to overlook the plain truth that only a quarter of such increase of the British population as there may be between now and the end of the century can be attributed to what they call "the present reproductive bonanza". The rest is simply a consequence of their own craft, which has now

made it possible for people to live longer and to survive a good many of the previously fatal hazards of middle life. So is it to be expected that the same people will band together in public to wring their hands about the once and for all increase of the British population which is likely to come about when, at some time in the next two decades, ways are found of treating or even preventing some forms of cancer?

The immediate trouble, of course, is that there is no means by which these statistics, meaningful or otherwise—and who can tell what the effects of British membership of the European Communities will be on the rate of outward migration?—can be related to what is elsewhere called the quality of life. The distinguished doctors say that on present trends, a quarter of the land surface of England and Wales "will be urbanized" by the end of the century and they say that "sheer overcrowding in cities with its attendant pollution" is a direct threat to mental and physical well-being. But is there any serious cause to fear that a country such as Britain will be necessarily worse off if fewer adults are killed off in middle age and if the numbers of old people are also increased? The calculation is not easily performed, but the chances are that the pattern of population in the year 2000 will be economically more productive than it is at present, and better able to support the sensible education of the young and the humane care of the old than at present. And what, in any case, is the evidence that overcrowding in cities produces "mental" afflictions among those who live there? To be sure, there are statistics to show that suicide rates are often higher in cities than in the country, but these are easily interpreted as consequences of the way in which the populations of cities are frequently overweighted with people of vulnerable ages. Furthermore, there are good reasons to believe that air pollution reduces longevity but there are also now reliable measurements to show that both in Britain and in the United States, air pollution in cities is decreasing. And in general, the charge that city life and the density of population in cities are hazards to health cannot be substantiated except by the misuse of facts.

What then is to be made of the constructive suggestions which the doctors put forward in their round robin? First, they ask that the government should be persuaded "that a population problem exists" so that "appropriate legislation is more likely to follow". What precisely is meant by appropriate legislation? Do the distinguished doctors have in mind more government help for family planning (in which case everybody will cheer)? Do they have in mind schemes such as that of the Duke of Edinburgh for a tax on large families (in which case most people will want to know more of the side effects of such proposals, most of which are still half-baked)? Or do they have in mind compulsory sterilization for the parents of large families, contraceptives in the drinking water (both of which have been discussed by Dr Ehrlich in connexion with the population of India) or do their



minds toy with the notion of sterilizing newborn children once a certain quota has been reached? The signatories of the round robin may think it unfair that such foolish motives should be attributed to them, but a vague call for "appropriate legislation" is likely to provoke precisely such curiosity. Indeed, that is the morbid kind of speculation which exaggerated declarations of crisis is likely to set in train. But few people will, of course, complain at the moderate proposals for an extension of family planning, with contraceptives on the National Health Service, the better conduct of abortions under the NHS and the "reappraisal of the laws and policies which govern the employment of women".

For people who say that the mass media, as they are called, must play an integral role in the whole operation, the doctors have been injudicious. They have addressed themselves to the wrong problem. In the first place, there is no reason to fear that a country such as Britain will find it more difficult in future than it has been in the past four decades to strike a reasonable balance between fecundity and death. By now, the influences which lead to larger families are comparatively well understood. Improved education is a step in the right direction, not merely because it makes people better able to look after their personal affairs but because it provides parents with an incentive to do the best they can for their children, something only easily possible when families are small. Prosperity also helps and, by extension, so does a narrowing of the gap between rich and poor.

But these are goals which many people hold to be desirable in themselves. Might it not have been prudent for the distinguished doctors to have cast their thoughts more widely to encompass these questions? There remains the difficulty of knowing what the population of a country such as Britain should be. In spite of a great deal of academic discussion in the past few years, it is now clear that there are no easy rules of thumb. One thing is however clear. If by some means the number of births each year were to decline more rapidly than it has done in the past few years, some decades

hence a smaller working population would have to support a still larger population of unproductive people than at present. To say that is not to imply that a reduction of the number of births each year is out of the question but merely that such a reduction would bring serious but calculated economic penalties.

The same unreflectiveness appears to have marred *The Ecologist's* "Blueprint for Survival". Those who have compiled it say that "the relevant information available has impressed upon us the extreme gravity of the global situation today". They foresee "the collapse of society" and consider that if present trends persist, "life support systems on this planet" will be irreversibly disrupted if not by the end of the century then "within the lifetime of our children". Governments, they say, are either refusing to face facts or are "briefing their scientists in such a way that their seriousness is played down". So, the argument goes, there must be a redefinition of the philosophy of civilized life and a restructuring of society as a whole.

The errors in this simplistic view of the present stage in the history of the human race are by now familiar. Much turns on the way in which industrialized societies are at present consuming raw materials at a substantial rate, and it is true that it seems increasingly unlikely that petroleum companies will be able indefinitely to discover new reserves at such a pace that future supplies are always ensured. Oil, indeed, may be the most vulnerable of the resources at present used, just as in Europe 2000 years ago native stands of timber proved not to be inexhaustible. But does it follow from this simple-minded calculation that there will come a time when, to everybody's surprise, petroleum deposits are worked out and industry is forced to grind to a halt? Is it not much more likely, about a century from now, that prices for petroleum will be found to be so high that even the least successful nuclear power companies will find themselves able to sell reactors more easily?

In the same way, is it not likely that the apparently impending scarcity of copper (belied for the time being by the obstinately low price at which the metal is at

Survival Signatories

A statement in *The Ecologist* says that the undersigned, without endorsing every detail, fully support the basic principles embodied in the *Blueprint for Survival* both in respect of the analysis of the problems we face today, and the solutions proposed.

Professor Don Arthur (King's College, London); Professor D. Bryce-Smith (University of Reading); Sir Frank Fraser Darling; Professor G. W. Dimbleby (Institute of Archaeology, London); Professor George Dunnet (University of Aberdeen); Dr P. N. Edmunds (Fife District Laboratory); Professor R. W. Edwards (University of Wales Institute of Science and Technology); Dr S. R. Eyre (University of Leeds); Professor Douglas Falconer (University of Edinburgh); Professor John Friend (University of Hull); Professor F. W. Grimes (Institute of Archaeology, University of London); Professor John Hawthorn (University of Strathclyde); Professor G. Melvyn Howe (University of Strathclyde); Sir Julian Huxley; Dr. David Lack (University of Oxford); Dr J. P. Lester (British Medical Association); Dr John A. Loraine (MRC Clinical Endocrinology Unit,

Edinburgh); Diana G. M. Loraine; Dr Aubrey Manning (University of Edinburgh); Professor Vincent Marks (University of Surrey); Professor Ivor Mills (University of Cambridge); Dr E. Mishan (London School of Economics and American University, Washington); Professor P. J. Newbould (The New University of Ulster); Professor Forbes W. Robertson (University of Aberdeen); Professor W. A. Robson (London School of Economics); Dr J. Rose (Institute of Environmental Sciences); Sir Edward Salisbury; Dr R. Scorer (Imperial College London); Peter Scott (Wild Fowl Trust); Dr Malcolm Slessor (University of Strathclyde); Professor C. H. Waddington (University of Edinburgh); Dr Watson (University of Strathclyde); Professor V. C. Wynne-Edwards (University of Aberdeen and Natural Environment Research Council).

present marketed) will encourage the use of aluminium as a conductor of electricity? To be sure, as the developing countries gather economic momentum, they will begin to make larger demands on raw materials such as these, yet it does not follow that they will have to repeat in every detail the industrial history of the countries now industrialized, and it remains a comforting truth that the raw materials on which the products of modern industry are based loom less large in economic terms than the products of the Industrial Revolution. Computers, after all, need very little copper for their manufacture. In general, the problem of raw materials is not a problem of the exploitation of a finite resource, however much it might be made to seem as such, but is a problem in economics—how best to regulate the prices of raw materials so as to balance the present demand against the probable demand in the future, how best to encourage what kinds of substitutions, how best to bring into production new reserves (not the least of which are the oceans of the world). Nobody should think that there is nothing to worry about. Good planetary housekeeping, as *The Ecologist* would no doubt describe it, should be an important objective of public policy. But it is a public disservice to describe such intricate and interesting problems in such simple and scarifying terms.

Similar fallacies attend *The Ecologist's* analysis of the supply of food. The document says that food production in the developing world has “barely kept abreast of population growth” and that such increases as there have been are a consequence of the “opening up of new land for cultivation”. It goes on to say that this will not be possible for much longer, for “all the good land in the world is now being farmed”. Factually, these statements are incorrect. In many parts of South-East Asia, the past few years have seen dramatic improvements in agricultural productivity, acre for acre. In any case, it remains a fact and even something about which agronomists should hang their heads that tropical regions are still comparatively unproductive of food. But the chief complaint of this declaration is that the “FAO programme to feed the world” depends on an intensification of agriculture and that the strains of wheat and rice likely to be the work horses of Asian agriculture are more vulnerable to disease and more demanding of fertilizer.

So what? must surely be the moderate reply. In North America and Western Europe, after all, agriculture is much more intensive than most agricultural practices likely to be common in Asia in the next few years. And the benefits of intensive agriculture are not merely that a given acre of land can produce more food each year but that it can be made to do so at a lower labour cost. Indeed, it might well be calculated that until the populations of the developing world are able to feed themselves without employing more than half of their labour force on the land, they will not be free to develop either along the lines of Western industrialization or along some other route that they might prefer. The fact that intensive agriculture entails crops which are highly specialized and therefore vulnerable to epidemic diseases of one kind or another is no more relevant in Asia than in, for example, North America.

The abiding fault in these discussions is their naivety, and nowhere is this more true than in speculations about the social consequences of the phenomena over which *The Ecologist* wrings its hands. Starting with the assertion

that the developed nations have already collared the raw materials with which developing nations might seek to improve their standards of living, the journal goes on to say that “we are altering people’s aspirations without providing the means for them to be satisfied. In the rush to industrialize, we break up communities, so that the controls which formerly regulated behaviour are destroyed. Urban drift is one result of this process, with a consequent rise in antisocial practices, crime, delinquency and so on. . . .” This is an echo of the distinguished doctors’ declaration about the consequences of crowding, but is it fair to describe this, as *The Ecologist* does, as a portent of the collapse of society? Is it reasonable to say that in such circumstances, “it is more than probable that governments will fall into the hands of reckless and unscrupulous elements, who will not hesitate to threaten neighbouring governments with attack if they feel they can wrest from them a larger share of the world’s vanishing resources”? The truth is, of course, that this is mere speculation. All the attempts which there have been in the past few years to discover correlations between such factors as population density and prosperity per head of population with the tendency to violence, either civil or international, have been fruitless. Who will say that the crowded Netherlands are more violent than the uncrowded United States? And who will say that the forces which have in the past 2000 years helped to make civilized communities more humane can now be dismissed from the calculation simply because a new generation of seers sees catastrophe in the tea leaves?

100 Years Ago



THE FOUNDATION OF A TECHNOLOGICAL EDUCATION*

TECHNOLOGICAL education is taken up by many writers on the subject at the time when a youth is supposed to enter the School of Technology; and scientific men, as a rule, do not seem to set sufficient stress upon the necessity of laying the foundation for it at a much earlier age. It is not indeed scientific men alone who are interested in this question, but they are the authorities who should speak out upon it, for they alone are competent to pronounce an opinion upon the value of scientific education. It cannot be expected that men who themselves know nothing of science, care nothing for its progress, and recognise none of the obligations under which they lie to it, should favour its introduction into our schools, and thus depart from the stereotyped and antiquated system of education, that brings up our youth but partially fitted or altogether unprepared for a majority of the occupations they are destined to pursue, and exposed at every point to suffer from their own ignorance and the impositions of others. Every one now-a-days should have such a knowledge of scientific principles and methods as will enable him to form a just idea of the value of science, and to distinguish between knowledge and pretence—between science and quackery.

From *Nature*, 5, 233, January 18, 1872

OLD WORLD

WATER RESOURCES

Untapped Wealth

THE trouble with water in Britain, as far as industry is concerned, is that it falls at the wrong time and in the wrong places. The alternative to transporting water to the cities is to attract industry to regions where water is plentiful. This is the approach now being used by the Eastern Borders Development Association (EBDA) which has realized that the great wealth of river water (from the Tweed) and underground water available near Berwick could be a carrot in persuading industry to develop the England-Scotland border region of Berwickshire, North Northumberland, and the County of Roxburgh.

From 1945 to 1966 the population of this region declined at 10 to 11 per cent in each ten year period, and by 1966 even the population of the towns was declining. EBDA was set up to counter this flow and has already succeeded in that an overall decline in population of only 6 per cent is projected for 1966-76, with the population of the towns actually increasing.

Colonel J. I. M. Smail, chairman of EBDA, said this week that conservative estimates indicate that ten million gallons of water can be extracted daily from the Tweed, and replaced after processing. In addition, underground water of great purity is also available. Existing boreholes supply 600,000 gallons daily, and it seems that further drilling could produce another 1.5 million gallons each day. This high quality water is particularly suitable for chemical and pharmaceutical processes, because it is not only pure but also has a high degree of temperature consistency and is free from turbidity.

But what of the effect of industry on the environment of the region? The area is one of considerable natural beauty and historic interest—indeed one of EBDA's long term projects is the development of a tourist industry. It seems, however, that local authorities are in close touch with EBDA and that a careful eye will be kept on the kind of industry allowed into the area. Also, the size of factory which the association wishes to encourage is planned to fit in with the social requirements of the region. The association is also operating a scheme whereby skilled workers who have been forced to leave the region are informed of new developments. So far 1,800 families have indicated a desire to return to the border region—some from as far away as Detroit—if work is available.

Will the guarantee of water be more successful than the other inducements to regional development?

INFORMATION RETRIEVAL

Only Connect

A SERVICE designed to give companies the benefit of the 40,000 technical reports written annually in research establishments in Britain and abroad is to be put on a self supporting basis. The service—known as Techlink—has been available free of charge for the last three years from the Technology Reports Centre, Orpington, to 5,000 companies. The Department of Trade and Industry, which is responsible for the service, has decided to make it available to all British companies on a subscription basis.

One page summaries of reports which may have industrial applications are prepared by a team of four technical experts and made available under 40 subject headings. Companies can then subscribe to as many of these categories as they wish at a minimum subscription

of £6 a year. For £25 a year they can receive all 40 categories. Over the past three years, Techlink has been issuing about five summaries a week and it is intended to increase the output.

It is expected that the service will become self supporting over the next three years and an advertising campaign intended to reach 42,000 companies is to be launched at the end of January to recruit subscribers.

Each of the summaries gives the source of the information and a name for further contact which, if a very popular process becomes available, could prove a strain on the laboratory responsible. The reports will be drawn from government laboratories, universities and research establishments, particularly in Britain and North America. The service should provide a cheap and efficient means for companies to keep up with innovations in their field without having to examine all the published reports.

Salary Slide

MANY graduate engineers seem to have lost the salary advantage that they had over graduate chemists in 1968, and the salaries of graduate engineers in general still lag behind those of physicists. These are two of the conclusions to be drawn from a survey conducted by the Council of Engineering Institutions (*The 1971 Survey of Professional Engineers*, £2.50), the latest of several surveys published recently by professional bodies such as the Royal Institute of Chemistry (*RIC Remuneration Survey 1971*, Pt. 1, 1971; £1.00) and the Institute of Physics (*Physics Bulletin*, 22, 663; 1971).

ment, re-training and the distribution of engineers by field of work. At the time of the survey (June 1971), national unemployment was running at about 3.3 per cent, but the proportion of engineers—both graduate and non-graduate—out of work was one per cent. Engineers over fifty years old were rather worse off, and 1.6 per cent of them were seeking re-employment.

In the preamble to the survey, the council singles out the disappointing level of re-training for special comment: "In a profession where technology is changing so fast, the fact that less than 7 per cent of engineers were subject to re-training and updating (between June 1970 and June 1971) should

Table 1 Median Salaries of Graduate Engineers, Chemists and Physicists in 1971

Age group* years	Engineers £	Chemists £	Physicists £
26-30	1,900	2,080	2,005
36-40	3,000	3,080	3,213
46-50	3,700	3,910	4,080
56-60	4,002	4,000	4,223
61-65	4,027	4,000	4,407

*In the survey of engineers the age groups were 25-29 and so on.

The median salaries of graduate engineers, chemists and physicists in 1971 are shown in Table 1. Engineers over the age of forty-five now earn about the same as or slightly less than chemists of the same age, by contrast with the situation in 1968 when engineers between fifty-five and sixty-five earned £200 to £300 more than their chemist colleagues. Graduate physicists now earn about 7 per cent more than graduate engineers.

Other matters covered in the engineering survey include unemploy-

ment, re-training and the distribution of

be viewed with some concern". The council also expresses concern that there are signs of a decrease in the proportion of engineers working in what it terms the "investment" areas of the economy—machine tool manufacture, research and education—which is matched by an increase in the number working in service industries such as transport and communications. The proportion of engineers employed in education fell from 8.5 per cent in 1968 to 8.1 per cent in 1971.

MARS

Lost in the Dust

from our Soviet Correspondent

THE partial disappointments of the current Soviet Mars investigations caused by the failure in transmission from the landing capsule from Mars 3 and by the ever-increasing dust-storms, which are now apparently seriously interfering not only with direct observations from the orbiting Mars 2 and 3 probes but also with supporting observations from Earth, do not seem to be causing the programme planners undue despondency. Press interviews given by leading figures in the programme, notably by Academician A. A. Blagonravov, head of the Soviet Commission on Space Exploration, give a highly optimistic view of the future of their Mars programme.

In outlining Soviet plans, Blagonravov stresses that for the immediate future, exploration of Mars will be by automata only. The success of Lunokhod, and the achievement, in Mars 2 and Mars 3 of automatic control systems that will function without command from Earth, have given a practical demonstration of the feasibility of this approach, in spite of the unexplained breakdown of transmissions from the Mars 3 landing capsule. Technical problems of fuel and payload, too, hints Blagonravov, will make a manned Mars mission feasible only when intermediate orbital stations (on the Tsiolkovskii plan) have been established. Moreover, microbiologist Vasilii M. Shilov (*Pravda*, January 1, 1972) says that the presence of microbes in the Martian atmosphere is "quite probable"—a further reason for confining future explorations to automatic devices. At least, he suggests, a sample of Martian soil should be brought back to Earth, after the manner of the Luna 16 experiment, before any manned mission is planned. This approach seems to suggest a change in policy from the mid-60s, when teams of trainee cosmonauts were confined under simulated spacecraft conditions for times comparable with a Mars expedition.

Yet even with its failures, the current programme is yielding some significant data. Writing in *Pravda*, Academician G. Petrov asserts that unlike the United States Mariner programme, which is primarily a photographic project, the Soviet probes carry devices to investigate relief, radiation and photometry.

The special equipment of the Mars probes, says Petrov, includes an infrared radiometer, consisting of a pair of miniaturized telescopes, one directed at the planet and one at space, capable of receiving infrared radiation from an object cooled to -100°C . The temperature measurements from Mars 3

(at an orbital distance of some 1,500 km) are averaged over a segment of diameter 30 km. The low-temperature range of this device allows accurate monitoring of the "night" as well as the "day" side of the planet. The new spectroscopic device for measuring relief is similar in principle to apparatus used in measurements from Earth, but whereas these could only distinguish features of 1,000 km and more wide, the new equipment can detect features down to 12 to 15 km.

Another special apparatus measures the brightness of the planet in the visible range, from 4000 to 7000 Å. A special study is being made in the brightness difference at the terminator. It is hoped that the data from this experiment will provide information concerning the aerosol (dust) content of the atmosphere and its nature.

OCEANOGRAPHY

Deep DivingFrom *La Recherche*

THE French Centre National pour l'Exploitation des Océans (CNEXO), in the persons of assistant director Jacques Perrot and associate Jacques Riffaud, recently held forth on CNEXO's accomplishments in 1971. Although the results may not be as spectacular as those of the preceding year, it is clear that CNEXO is now getting its second wind and progressing with some interesting projects.

The experimental operations Janus 2 and Phyllis 1 established new world records for deep diving, whereas projects Belouga, Sagittaire and Saturation 3 were useful in both human and animal physiological research. And as well as the Océan programme theme to develop coastal and continental shelf areas under water, CNEXO signed two important exploration contracts with the Compagnie Maritime d'Expertises (COMEX) of Marseilles and Captain Jacques-Yves Cousteau's Centre d'Etudes Marines Avancées (CEMA).

The Belouga operation consisted of four simulated dives to depths of between 100 and 300 metres at -2°C , providing the necessary setting to study heat transfer and epidermal heat loss. Data gathered in this operation have already shown that divers engaged in petroleum research and drilling can operate in cold seas. In the Sagittaire tests, held from November 9 to December 3, 1971, under the auspices of COMEX, four men stayed for a long time at a simulated depth of 300 metres inside the COMEX habitat at Marseilles. One of the men ran tests on his osmotic pressure and the effects of the ambient conditions on nervous tension. The purpose of the Saturation 3 scheme, run at CEMA's High Pressure Physiology Laboratory between December 6 and

December 20, 1971, was to study the effects of pressure on the dissolution of gases in the blood stream on two subjects who had been exposed to a simulated depth of 500 metres for 24 hours.

Whereas all the results of these experiments will not be available for some months yet, there remains a need to repeat the tests. It will thus not be realistic to "dive" as deeply as 600 metres during 1972; indeed, such a depth may well not be attainable until 1975 or later. Research operations of this type are clearly designed to meet the needs of both the petroleum industry and the military establishment.

In a more strictly scientific framework, CNEXO has announced plans for a large-scale Franco-American operation when the French will join forces in 1973 with the Woods Hole oceanographic research centre in Massachusetts to develop FAMOUS, the French-American Mid Oceanic Underwater Survey. This programme is for forty or so dives during the summer of 1973 just south of the Azores, using Alvin, the bathyscape and the diving saucer as the immersion vehicles. Geophysicists consider this research as highly important, because it will permit on-site study of the vertical movement mechanisms of the material composing the Earth's mantle and compilation of a precise stratigraphy of rift faces by means of core sampling. There is also a magnetism research plan and a relief study of an 8 km strip of the sea bed, to be recorded by television.

The 1973 research voyage will be preceded in 1972 by a programme of hydrographic charting and exploration. Financing of FAMOUS will come, on the American side, from the National Science Foundation, the National Oceanic and Atmospheric Association, and the United States Navy; on the French side, from CNEXO and the French Navy.

Royal Society

PROFESSOR SIR ALAN HODGKIN, President of the Royal Society, has appointed eight vice-presidents for the coming year. These are Sir Frederick Bawden, director of Rothamsted Experimental Station; Sir Bernard Katz, University College, London; Sir Harrie Massey, University College, London; Professor K. C. Dunham, University of Durham and Institute of Geological Sciences; Professor E. J. W. Barrington, University of Nottingham; Dr H. M. Finnieston, British Steel Corporation; Professor R. A. Gregory, University of Liverpool; and Professor R. V. Jones, University of Aberdeen.

NEW WORLD

Nixon Backs Economy Shuttle

by our Washington Correspondent

WHEN President Nixon announced last week that he has given his personal blessing to the space shuttle, it was widely acclaimed as a victory for the National Aeronautics and Space Administration. For long the project on which the agency and the ailing aerospace industry have pinned their hopes for the 1970s, the shuttle has been the subject of long and at times acrimonious debate between NASA officials and the Office of Management and Budget.

But last week's announcement bears witness to the sacrifices the agency had to make to get its project accepted at all. The \$5,500 million mentioned by President Nixon is only little more than half the figure asked for by NASA and this has forced some radical rethinking about the project's design. The booster will now no longer be manned, and at best it will be only partially recoverable—a factor which has considerably altered the economic basis on which the agency has consistently sought to justify the project. A less obvious casualty of the agency's victory over the Office of Management and Budget is the extent to which scientific projects such as the Grand Tour of the outer planets have been sacrificed.

The announcement, couched in flowery terms emphasizing the benefits that space research has brought to life on Earth, marked the end of one battle and the beginning of another. Having convinced the Office of Management and Budget of the worth of the project (partly by a direct representation by NASA's Administrator James Fletcher to President Nixon), NASA officials must now convince members of Congress that \$5,500 million, plus a 20 per cent "contingency factor", will be well spent. In a sense, President Nixon's announcement was both a challenge to his Congressional opponents and a move designed to link his personal support with a project likely to create up to 50,000 jobs in the aerospace industry, many of which will be in the valuable electoral state of California. The announcement also fortuitously came out on the day that the pay board rejected a wage claim from aerospace workers.

The budgetary stringencies that have forced the cost of the project down from the \$9,000 million that was being talked about by NASA officials, and which many critics insist would have been considerably higher, have affected chiefly the booster. The orbiting part of the shuttle will remain essentially as planned,

and importantly, the payload capacity has not been compromised—the shuttle is still expected to carry 65,000 pounds of scientific payload, and a four-man crew consisting of two pilots and two to look after the payload. NASA is also studying designs which will allow up to twelve men to be carried in place of the scientific payload.

The orbiter, which is designed to separate from the booster stage at a height of about 190,000 feet, should be able to remain in a low orbit for up to thirty days, and return to its launching site. This will require a complex set of aerodynamical criteria to be met, and it is this aspect of the development that is expected to push up the costs as a result of unforeseen engineering difficulties.

But, while the orbiter remains essentially the same, designs for the booster have been thrown back into the melting pot. NASA has already dropped the original concept of a fully re-usable booster, which would be piloted back to the launching pad, checked and refuelled, and the agency is now undecided whether to go for a totally expendable booster or one which would be dropped into the ocean, recovered, cleaned up and re-used. To a great extent, the final decision will rest on the choice of propellant. If a solid fuel is used, the booster would certainly be expendable, but the agency is also considering plans for a new design of rocket motor in which hydrogen and oxygen would be fed into the motor from pressurized tanks, rather than pumped in as original plans entailed. A pressure-fed booster might be made recoverable.

What this means is that by using existing technology in a solid propelled booster, or more easily accessible technology in a pressure fed booster, NASA can considerably reduce the research and development costs on the shuttle, which means that the peak expenditure during the mid-1970s will not push the agency's budget up to a politically unpalatable level. But while gaining on the research and development costs, a shuttle with an expendable or only partially recoverable booster would cost more per flight to operate. The original plans envisaged a launch cost of between \$4.5 and \$5.0 million, but NASA officials are now suggesting that it will cost about \$10 million to launch one shuttle mission. These cost adjustments have considerably upset the economic arguments with which NASA has in the past sought to justify the project, although the agency

still insists that the launch costs with the shuttle will be considerably less than with fully expendable launchers.

Three studies of the economics of the shuttle, all published last year, emerged with rather different conclusions. One, conducted by the Rand Corporation for the Air Force, concluded that the shuttle would at best result in marginal savings, and at worst would cost \$5,000 million more than conventional hardware between 1970 and 1990. Another, conducted by Mathematica Inc. under contract to NASA, concluded that the shuttle would save money even if it cost up to \$18,000 million to develop. Finally, a panel appointed by the Federation of American Scientists concluded that the shuttle would be economically justified only by a manned programme several times larger than the Apollo programme, measured in tons orbited per year. (Similarly, the Space Science Board of the National Academy of Sciences said in a report on priorities in space research published early last year, "it is clear that space science and applications by themselves are insufficient to justify the cost of developing the shuttle".)

The change in costs for the revamped shuttle have further confused the already murky economic picture, for as one NASA official suggested last week, the basis of previous studies has been invalidated. But in a press conference immediately after President Nixon's announcement, Dr Fletcher told reporters that the shuttle would bring down the cost per pound of launching a satellite from about \$700 to about \$100. But the critical factor is whether the launch rate would be sufficient to offset the costs of research and development. That is a question which so far has not been answered.

One obvious facet of the announcement of President Nixon's support of the shuttle last week is that it was a political move. For one thing, the announcement has not altered the status of the project—it had, as recent speeches made by Dr Fletcher will testify, already become the chief goal of NASA's manned spaceflight programme and it is seen as a shot in the arm to revive the flagging aerospace industry suffering from the abortion of the Apollo programme. The programme must also get Congressional approval, and a more obvious forum for announcing the Administration's commitment to it would have been in the budget message to be transmitted to Congress later this

month. In short, the shuttle has become to President Nixon what the Apollo programme was to President Kennedy in the early 1960s—a political goal.

Congressional opponents of the shuttle, who in previous years have been unable to shoot down the project's budget, are already girding their loins for the ensuing battle. NASA's budget for 1973 is expected to contain about \$200 million in funding for further design studies and contracts for the shuttle, which is still not a very tempting target (the Administration asked for only \$100 million last year), and Senator Edmund S. Muskie, President Nixon's most likely Democratic opponent in November, has already weighed in with a statement saying that the project should not be given priority since there are other more pressing demands on scarce resources.

But Muskie's comments look pale against those of Senator Walter F. Mondale, chief opponent of the shuttle in the Senate in the past two years. He said last week that Nixon's decision to back the project is "yet another example of perverse priorities and colossal waste in government spending. Typically this Administration can squander \$6,500 million to fly four people in orbit, while it refuses less than one third that amount to provide desperately needed day care and development programs for millions of pre-school children". Allied with Senator William Proxmire, Mondale plans to mount a full scale assault on the shuttle's budget, and comparisons with the SST are already being offered.

Another highly political aspect of the shuttle is that it is expected to create about 50,000 jobs in the aerospace industry—about a quarter of those lost through running down the Apollo programme. And in addition, selection of the launch base will be politically sensitive since it would be a strong economic asset to the state in which it is located. The decision to scrap plans for a fully re-usable booster has, however, effectively narrowed the site selection down to Cape Kennedy or Vandenberg Air Force Base in California, since those are the only sites in which populated areas would not lie under the flight path of the booster. Moreover, since Dr Fletcher said that only \$300 million would be spent on site preparation, Cape Kennedy seems to be the chief contender; its existing facilities would not require very extensive modification.

AEC

In Court Again

by our Washington Correspondent

THE Atomic Energy Commission, still in turmoil after the court of appeals forced it to amend its environmental rules, and still in the throes of a management shake-up instituted by the new

chairman, James R. Schlesinger, is now faced with an even greater threat to its existence. Last week, a group of six environmentalist organizations filed a suit in the district court alleging that the act which set up the commission is unconstitutional since it created an organization which both promotes and regulates the atomic energy industry. The suit charges that the dual role of the commission denies the citizen his constitutional right to a free hearing on nuclear matters, and "violates the due process principle that no man shall judge his own cause".

The environmentalist groups, headed by the Conservation Society of Southern Vermont, which has been active in bringing public attention to the possible environmental and health hazards of the Vermont Yankee power station on the Connecticut River, want the commission stripped of one of its roles, but as yet they have no set convictions about which role should be divested and who should take it on. But they are convinced of one thing, that until the case is settled no new licences for construction or operation of new plants should be issued by the commission, and the groups will ask the court for a temporary injunction to that effect.

But the environmentalists are not alone in realizing the potential conflict of interest inherent in the commission's dual role. The AEC itself decided in 1961 to separate promotion and regulation into different divisions, and in a recent address Dr Schlesinger handed out a stern warning to the nuclear industry that the commission would no longer fight its battles (see *Nature*, 233, 582; 1971). Also, in a speech made last November, James Ramey, one of the five commissioners, suggested that the time has come for the AEC and the Joint Committee on Atomic Energy to take another look at the commission's dual role.

But Irving Like, an attorney for one of the environmentalist groups, said last week that the separation of interests inside the commission is a fiction. "The decision-making machinery is so infected with promotional bias", he said, "that the AEC is not capable of adjudicating disputes." The twenty-seven-page brief filed with the district court also charges the commission with displaying open bias in a number of regulatory processes. It is alleged, for example, that the commission entered into a contract with Vermont Yankee for fuel enrichment before hearings on an operating licence took place, and that the commission's licensing regulations allow manufacturers to purchase or make major components of the steam generators before a construction permit is granted, thereby ruling out a review by the commission of the safety factors of such components.

Asked last week how far the conservation societies are prepared to take the case, Peter Strong, president of the Conservation Society of Southern Vermont, said "to the Supreme Court if necessary". Mr Strong is hoping that the case will bring together a large coalition of environmentalist groups prepared to chip in most of the costs. The whole process could cost up to \$150,000.

AUTOMOBILES

Standards Unmet

by our Washington Correspondent

A COMMITTEE of the National Academy of Sciences has put forward the tentative conclusion that motor manufacturers will not be able to meet the requirements of the Clean Air Act of 1970. The committee, in its first semi-annual report to Congress and to the Administrator of the Environmental Protection Agency, suggests that 1975 model cars will not be able to meet the exhaust emission requirements specified by the act without replacement of catalysts during the lifetime of the vehicle as specified by the act. Moreover the committee suggests that if the requirements of the act were met, cars would cost about \$200 more to buy, up to 12 per cent more to run, and they would not perform as well as present cars.

But the committee suggests that if manufacturers were given one more year to develop engines to meet the requirements, the reliability of the vehicles would be markedly improved. And, as far as controls on emission of oxides of nitrogen are concerned, the committee paints an even gloomier picture. "The technology of catalysts suitable for NO_x reduction is not nearly as advanced as that of oxidation catalysts," the committee states, and "the level of current research and development on reduction catalysts for NO_x control is not commensurate with the importance of the problem".

The committee's report is potentially very influential, since the Clean Air Act specifies that the Administrator of the EPA has the power to defer compliance for one year, but in doing so, he must take into account advice from the academy.

The committee says that it addressed itself specifically to the question of whether the motor manufacturers would have the technology available to meet the requirements by 1975. It bases its conclusions on information supplied chiefly from industry, but more than 700 statements were also sent to newspapers, individuals, magazines, environmental groups and publications. But the committee claims that the response has been "disappointingly small".

Lord Rothschild and Industry

The following is an industrialist's view of the Rothschild and Dainton reports. The contributor is Dr E. Eastwood, General Electric Company Ltd.

IN January 1970, Mr Anthony Wedgwood Benn, then Minister of Technology, issued his green paper on *Industrial Research and Development in Government Laboratories*. This document proposed the setting up of a National Research and Development Corporation to provide unified management of the research resources locked up in government laboratories which could then be deployed for the solution of industry's problems—on a customer-contractor basis. These proposals found no support in industry which could see little advantage in a reorganization of government laboratories that failed to remove the burden of overcapacity.

The present green paper, *A Framework for Government Research and Development*, concerns itself with those aspects of government science not covered by its predecessor; will it be better received?

Of course it will be pointed out that it was not the purpose of this green paper to deal explicitly with industry's interest in government research and development. But at such a time as this, when the economy is in grave peril, it hardly seems appropriate for the paper to be so exclusively concerned with the restructuring of a portion of government science. For only brief mention is made of the research and development affairs of the Ministry of Defence or of the Department of Trade and Industry, although these two bodies absorb 72 per cent of the total departmental research and development expenditure. It is the research council system set up within DES in 1965 which receives closest examination. Expenditure under this heading is running at £109.5 million which represents 17 per cent of the total departmental research and development. It is suggested that £27.7 million of this £109.5 million can be identified as applied science of direct interest to user departments such as DHSS which should sponsor such work with the research councils (or elsewhere!) on a customer-contractor basis. It should be remembered that this invocation of "customer pressure" in user departments of state through control of applied research funds was also suggested in the green paper of 1970.

The Dainton report argues that evolutionary development of the present

well proven research council system will be sufficient to adapt it to modern needs. That user departments such as DHSS and SHHD should have some voice in framing the programmes of the Medical Research Council, for example, is recognized and to meet this need Dainton proposes that they should be represented on a new Board of the Research Councils. It is stated that the structure advocated "would enable the research councils to play a still more effective part in making progress towards national goals". Of all such goals, none can be more important than increasing the strength of British industry, but nowhere does the report show how this will be achieved by the approach to reorganization advocated.

Rothschild recognizes that science enters intimately and decisively into the affairs of most major government departments which he regards as customers for applied science effort, whether provided by the research councils or elsewhere. His report is based firmly on the principle that applied research and development has a practical application as its objective which is recognized and defined before commencement of the work—such work should be performed on a customer-contractor basis. "The customer says what he wants and pays for it—the contractor executes the project."

In industry most research and development is of the applied type with the operating company as the customer. The product that emerges has to be right in performance, price and timeliness if it is to succeed in the markets of the world. The user departments of government only partly fill this customer role; in particular, the market-place test of the product is lacking. Neither does the customer always know best what applied programmes should be mounted. Certainly he knows what he can afford. But the operating company in industry often chooses its product programmes from the various possibilities offered to it by the laboratories which have done sufficient research work to establish feasibility and the economics of development, production and application. The relationship between the DHSS and the MRC is likely to be much the same but if it is to be successful there must exist the strong Chief Scientist organization postulated by Rothschild and how is

this to remain effective if it is staffed by non-practising scientists?

The SRC and SSRC are excluded from the proposed new organization although a significant rider states that their activities will be "studied in due course". It is to be hoped that it will be borne in mind that a vital function performed by SRC is the postgraduate training of scientists and engineers. The maintenance of a supply of well trained men is probably the most important contribution made by SRC to industry rather than the output of university applied science handled by its boards, valuable though this work is.

It is difficult to withstand the logic of the Rothschild proposals but it should be noted that they do not spell out the criteria determining the amount of financial support to be given to civil science in the future. Rothschild's view, expressed in his Royal Society of Arts lecture of December 8, 1971, is that it is parliament's job to weigh the social desirability of science against other contenders for shares of the national purse—the clear implication being that the proportion devoted to science in the future would be reduced.

It is a strong criticism of both reports that they do not deal with the real problem facing the nation, which is to produce a healthy economy, and it is difficult to see how any of the proposals will benefit British industry. Government science is, apparently, to continue to be a heavy overhead. If the user departments are to be able to go to industry for the applied science services they require, as is implied in certain paragraphs of the report, then some useful purpose will be served—but not if industrial laboratories are to be subject to redundancy and government and university laboratories are not.

Rothschild is looking for a way in which the efficiency of government research and development can be maximized. To identify that part of the work which is "applied" and close to the industrial concept of applied research and development, and to organize it on similar customer-contractor lines, is sound sense and should be implemented even though it might apply only to a comparatively small part of the government's total expenditure on research and development. A successful precedent for such a sponsoring arrangement is the work placed by the MOD with universities, particularly in the field of electronics performed under the aegis of the CVD organization. This scheme is welcomed by the universities and much valuable applied science is performed in this way.

SCIENCE POLICY

Shadow of Rothschild over Strathclyde

by our Special Correspondent

THE meeting organized by the Council for Scientific Policy at the University of Strathclyde last week on the problems facing university science was overshadowed by the government green paper published last November which included the reports of Lord Rothschild on the organization and management of government research and development and the Council for Scientific Policy's report on the future of the research system. The green paper understandably dominated the conference, although the research councils as such occupied only a part of the organized sessions.

Research Council System

The sessions on the research council system, the chief attraction at the conference, were marked by the eloquence of Sir Frederick Dainton, the champion of the research councils in the tournament with Rothschild. He said that science was necessary to satisfy basic human curiosity and that "one must not quench curiosity or man will retrogress". "Science", Sir Frederick continued, "gives man power over his environment and regrettably over his fellow man."

The public attitude to science has shown two distinct phases since 1945. First, according to Sir Frederick, there was the massive investment in science that followed the Second World War, based on the belief that "if the clever scientist is given what he wants, then all will be well". Since 1965, however, there have been growing doubts that the scientist is not producing the goods—evidenced by the growing concern of people who cannot see science producing solutions to the problems of pollution and who are afraid of the developments loosely called genetic engineering.

Since 1965, it has been clear that the science budget could not grow faster than the Gross National Product unless it could be shown that there was no connexion between the two. The present situation, according to Sir Frederick, is that Britain must first decide how much it wants to put aside for scientific activity, then decide who foots the bill and how scientific activity is to be managed.

Sir Frederick was opposed to drastic changes "unless it can be demonstrated without doubt where the existing system fails". The alternative is to adapt

the present system rather than make superficial operations.

Perhaps one of the most surprising aspects of this session was the general acceptance of the customer-contractor principle for applied research. Sir Frederick said that this principle was right, a view shared by Sir Brian Flowers. There was only one dissenter—Professor Patricia Lindop (University of London). But agreement in principle did not stifle widespread disagreement with Lord Rothschild's application of the principle.

Sir Frederick justified his acceptance of the customer-contractor principle by pointing out that all Britain's affairs "involve some degree of interdependence—requests from people to organizations or *vice versa*. The nation writes contracts continually with government departments, for example, the Department of Education and Science has a contract with the nation to provide education for children between the ages of 5 and 15 (soon to be 16) and has another contract for higher education between the ages of 16 and 18". He went on to say that the transfer of this thinking to research "needs careful review to see if the method needs to be and can be reviewed". He saw no problem with its application to what he calls tactical research—defined in his own report for the Council for Scientific Policy as "the science and its application and development needed by departments of state and by industry to further their immediate executive or commercial functions".

The difficulty arises in the grey area of research where the objectives are not so well defined and which Sir Frederick defined as being "intermediate in time and character" between basic and tactical science. In this area, the mere writing of a contract is not enough and it is very difficult to draw a line. So Sir Frederick held that "we should avoid inadequately informed decisions and avoid conflicts over boundaries".

Sir Brian Flowers opened the discussion by challenging the government on its stated intention of allowing "wide public debate" over the issues involved in Lord Rothschild's report. Moreover, according to his calculations, there is a discrepancy of a factor of three between the figures given by Lord Rothschild in Table 4 of his report (the recommended

percentages of the budgets of the research councils that would under the new system be transferred to the appropriate department) and that evaluated on the basis of the definition of applied research as given by Rothschild himself. According to Sir Brian, the sum that should be transferred to government departments is £5-£10 million to be compared with the £27.6 million quoted by Lord Rothschild in his report. Had Lord Rothschild applied his principle to the grey area of strategic research—an area specifically ruled out by the definition given in his report?

Sir Brian said: "The discrepancy of a factor of three or so together with the absurd time scales laid down by the government for serious discussion of important issues—even with the whole month of extension—leads one to the suspicion that consultation is not seriously intended. My question to the government is whether consultations over orders of magnitude are intended."

Mr P. R. Odgers (Department of Education) proclaimed that "this document is certainly published with a view to genuine consultation. The government has made it clear from the outset that only one part of the green paper—paragraph 5—has any conclusions in it. The report and figures are open to consultation". Professor A. W. Merrison (University of Bristol) continued the attack on the DES by saying that it was not enough for Mr Odgers merely to confirm the content of the



Professor Sir Frederick Dainton, chairman of the Council for Scientific Policy.

green paper. There was really a need for longer than two months for discussion and that "if the sincerity of the Department of Education and Science is not in doubt, then their knowledge and ignorance are".

Lord Rothschild's report received a further battering with Professor Patricia Lindop saying that Table 4, together with its associated appendix was "plainly wrong and the Rothschild report was beneath the level of what one can consider to be a serious document". Sir Brian Flowers had already complained that the report reminded him of a PhD thesis "that contained much good and original work but unfortunately the good work was not original and the original work was not good". Lord Rothschild's ears are no doubt burning most of the time.

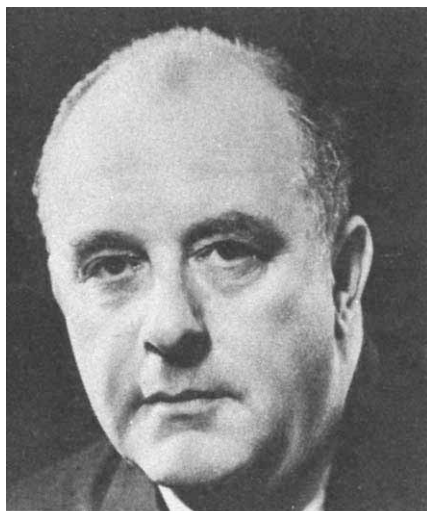
The conference was, however, not barren of constructive replies to Rothschild. Sir Brian Flowers thought that there was some good in both the Rothschild and Dainton reports and one would "do well to choose the best in both without being prejudiced and get a good system". Professor D. Johns (University of Loughborough) was concerned that if the customer-contractor system was instituted then university departments would become customers so that in the end there would be no need for research councils.

Professor F. H. Stewart, of the Natural Environment Research Council was worried that an "arbitrary decision would be reached". He said that there was not enough liaison between the research councils and government and that there should be more government representation on the committees within the research councils themselves. He complained that Table 4 of the Rothschild report was the "crux of the matter" and asked for an explanation of the discrepancy mentioned earlier.

Professor M. Fleischmann (University of Southampton) was concerned that the Science Research Council might be subjected to an investigation similar to that suffered by the other three research councils.

Sir Brian Flowers said that indeed the Rothschild principles were being looked at in connexion with the Science Research Council but that only a small amount of money was involved. The Department of Trade and Industry, he said, would like to play a greater part in affairs of the engineering board. He went on to raise the interesting question of what in fact is a customer in the sense implied by Lord Rothschild.

Is a customer really a person or a group who "knows what he wants and can pay for it"? Sir Brian pointed out that in the case of medical research the doctor or the patient cannot be considered as the customer as they do not pay. Therefore the department—in this



Lord Rothschild

case the Department of Health—is the customer. With this established fact, Sir Brian argued that the research councils themselves could be regarded as customers, but Lord Rothschild—according to Sir Brian—does not agree with this point of view.

Sir Alan Hodgkin, president of the Royal Society, praised the research council system and set going a listing of the ways in which modifications to the present system described by Lord Rothschild would be detrimental to the course of research in medicine, agriculture and the environment. Professor R. C. O. Matthews, of the Social Science Research Council, neatly summarized the feelings on the Rothschild proposals when he compared the scientific and legal establishments in Britain. He pointed out that government departments could do the work of the courts but an independent judiciary existed and that there had been no suggestion that this be done away with.

Dr O. Simpson, of the Cabinet Office, reiterated that the government had so far only made up its mind on the acceptance of the customer-contractor principle. He said he was not at the conference to pass on Lord Rothschild's views—"Lord Rothschild is the only person qualified to interpret the report". Dr Simpson added that a distinct advantage of having strong research and development within a government department would be that any supported project would be less vulnerable to financial squeezes. These assurances, together with those of Mr Odgers, should ensure that the united front against the Rothschild report that was apparent at the conference will be given a fair hearing even if there were no further extension of the time scale.

The final word in this session came, as it should have done, from Sir Frederick Dainton. He warned against the acceptance of a simple solution until more is known of the nature of the problem.

Science and Technology

The theory that there is a direct link between discoveries in pure science and technological development came under heavy attack from Professor H. G. Johnson (LSE) and Dr M. Gibbons (University of Manchester). Five years ago the theory was the foundation stone of the investigation by both people into the link between science and technology, but now they have concluded that the theory is wrong and that the economic benefits of pure research are indirect and difficult to assess. This recantation was not, however, entirely popular and several counter-examples were provided—the discovery of gas in the North Sea, for example. Professor Johnson was unmoved—the engineering developments behind the drilling operation were the products of basic research in quite different fields.

Dr Gibbons outlined to a sceptical audience the problems facing the economist who sets out to develop a model of the connexion between science and technology. Professor Patricia Lindop (University of London) felt that no evidence had been given to show that the economists were even remotely in a position to think of generating such models; but Dr Gibbons cited two United States studies as evidence that something can be done. One of these studies, for the Department of Defense on development of weapons, concluded that only 0.3 per cent of technological end-products can be attributed to discoveries in basic research, but a report commissioned by the National Science Foundation attributed 70 per cent of new discoveries to basic research. Dr Gibbons suggested that this alarming discrepancy could chiefly be due to what he called the arbitrary cut-off in the number of years that the economists had gone back in the literature to look for basic discoveries. Half a century was not too long.

British attempts to quantify the economic benefit of scientific research consist largely of the Byatt-Cohen report of 1969, which sets out to measure the benefit of a scientific discovery by the cash that accrues from it. The results, according to Professor Johnson, had shown that such problems cannot be studied so simply. Several studies had been undertaken since 1969 to test Byatt and Cohen's hypothesis but few definite conclusions were available at present.

Dr Gibbons sees science and technology as "two communities proceeding along two lines that from time to time communicate with each other". He also feels that "technology can build on technology", which implies that there is another arm to the science-technology model—that of technological development arising from other technologies

without direct recourse to basic discoveries—the development of the transistor is one of the examples that Dr Gibbons advanced.

So why not abandon basic research? This radical question was asked by Dr J. B. Adams, director-general of the 300 GeV project at CERN. Technological developments would still occur, according to Professor Johnson, for a pool of knowledge would still be available from other countries, and Britain could always “let the United States, for example, train nuclear physicists or Britain could hire scientists” from other countries if required. According to Johnson and Gibbons, however, no serious thought has yet been given to this particular heresy—thinking the unthinkable is not all that fashionable.

Opportunities for PhDs

What of the people produced by basic research? Mr J. S. Gough, of the personnel division of Imperial Chemical Industries, emphasized during his address of the industrial demands and opportunities for PhDs that ICI is not interested in a PhD degree as such—“or even MSc or BSc come to that”—but that it was interested in the person behind that degree. This view was sharply contradicted in a later session by Mr G. C. Maitland, a PhD student from the department of no less a person than Professor Dainton at Oxford. He said that from his own experience and from the experience of his acquaintances, industries were not that dispassionate and that job opportunities depended on how the candidate's previous research fitted into the needs and aims of potential employers.

Mr Gough, the sole representative of industry to address the conference, would not make any predictions on the future needs of scientists in ICI or in industry in general. “The demand for a PhD scientist,” he said, “is low at the moment,” and he saw no prospect, in the immediate future or on a longer time scale, of the demand increasing. He said that industry would never recruit such large numbers as in the past—“the bonanza period of the mid-1960s is passed” and, according to Mr Gough, there will probably never be a return to such a boom period again.

Mr N. T. Scott, the employment officer at the University of Nottingham, painted the same gloomy picture of the prospects of PhDs. We were now in the “post-Robbins era of higher education”.

In the 1960s, the number of first degrees in science awarded in Britain doubled, with 10,200 awarded in 1970. According to Mr Scott, the A-levels awarded in mathematics and science have steadily decreased since 1965. Mr Scott said, “This is the rot at the root

that will kill scientific education,” and this and the far from successful drive to get graduates to enter school teaching are considered by Scott to be “omens of disaster”.

The signs for the future, according to Mr Scott, “are even more gloomy”. Opportunities for science graduates during 1972 will be a little better than in 1971 except for the PhDs, of whom industry will need fewer. Sir Brian Flowers tried to obtain an estimate of the scientists needed by industry in ten to fifteen years' time—a factor, he said, that is of critical importance now that decisions are to be taken of the financing of the next UGC quinquennium.

Several unanswered questions from the session on the employment of PhDs were taken up by Professor W. Hayes (University of Edinburgh) Professor F. R. Bradbury (University of Stirling) and Mr G. C. Maitland (University of Oxford). They gave their views on the opportunities of the postgraduate system for broadening PhDs' horizons, Professor Hayes setting out a pilot scheme for the Department of Molecular Biology at Edinburgh, Professor Bradbury relating his experiences of broad-based PhD courses at Stirling and elsewhere and Mr Maitland telling the conference about what it is really like for a PhD student. With several exciting multidisciplinary courses now in existence or being set up, it became clear at the conference that the universities are slowly learning that there is more to a PhD course than mere research training.

So will the universities fail more of their PhD students? Professor M. M. Swann (University of Edinburgh) asked that they should but Sir Frederick Dainton said, although few PhD students did actually fail their final PhD examination, the high drop-out rate before the examination stage was in his opinion comparable with the failure rate for a first degree.

Science and Social Priorities

Dr J. B. Adams of CERN talked about social priorities in science policy. He took as his text the OECD report on *Science, Growth and Society*—known as the Brooks report after the chairman of the science policy group that constructed the report. Dr Adams, a member of the group, reminded the audience that the theme of the report was that science policy cannot be treated separately from economic and social issues.

Contrary to Sir Frederick Dainton's view that post-war science can be divided into two sections, the Brooks report sees science since 1945 in three distinct phases. First, the period until 1960 was “characterized by high public faith in the efficacy of science”. The

second period lasted until 1967 and was characterized by the gradual emergence of economists and systems analysts as significant influences in science policy. The third phase, in which we are now, is described as “the period of disenchantment of science and technology”. According to Dr Adams, “the scientific enterprise is not going down the drain. When an organism fights disease it will acquire antibodies to counteract the disease”. The report was accepted by all the science ministers of the OECD countries and this brought several comments from the audience.

Dr W. F. Williams of the University of Leeds was convinced that the acceptance of the report did not mean that its recommendations were not going to be implemented and that accepting the report amounted to burying it—as had happened to several reports on education in Britain in the last 30 years.

According to Sir Kenneth Berrill, chairman of the University Grants Committee, the Brooks report did not create any new problems or new situations. According to Sir Kenneth there are three main problems facing society—pollution, population and policy—and research should deal with these.

Professor M. M. Swann (University of Edinburgh) wanted to know why all the OECD governments had accepted the Brooks report without question. He ventured the suggestion that “the more vague a document is, the greater chance of it being accepted”. Swann said that the report was so vague that it could “mean anything to anybody”. Perhaps he came nearer the truth when he said, “What is needed is for the scientists to get on and do some science”.

Epilogue

The final word at the conference went to Dr A. M. Smith (University of Manchester Institute of Science and Technology) who claimed to be an outside observer. He complained that the conference had been remarkably conservative and that the university people present had “adopted a superior attitude to economists and analysts”. The conference listened politely to Mr Gough who in Dr Smith's view “made sense”, but he had little impact on the gathering. The real problems facing university science today, according to Dr Smith, were the “grievous lack of scholarships” mentioned by Sir Brian Flowers. The prevalent attitude was that the ills of society could be laid at the door of university scientists and the impetus of university education. These problems were real but had had little impact on the conference. In fact, said Dr Smith, the conference showed “the arrogant occupation of university scientists with preserving the *status quo*”.

NEWS AND VIEWS

How News and Views are Compiled

FOR the past several years, the section of *Nature* appearing under the heading News and Views has been concerned principally to provide readers with news of and commentary on developments of interest to professional scientists in all disciplines. One objective has simply been to inform. Although *Nature* is proud of the frequency with which important discoveries appear first in these pages, there are of course a great many important articles which appear elsewhere which, in the judgment of the staff of *Nature*, deserve to be brought to the attention of readers, especially those who look to journals like this for a view of how science as a whole is growing. At the same time, it is hoped to provide specialists in a number of important and quickly developing fields of research with an informal guide to the literature which is of most immediate concern to them. But this section has also included comments on articles appearing in *Nature* or in *Nature Physical Science* and *Nature New Biology*.

Most often, the comments on articles which appear in *Nature* have been designed so as to make the significance of some new development apparent to an audience wider than that of the specialists immediately concerned but also to draw attention, in a way which authors are frequently prevented by their functions from doing, to the probable directions of future developments. The comments on the papers which appear in the Monday and Wednesday editions of the journal are, by contrast, meant as a means by which readers of the principal edition of *Nature* can learn of important developments due to be announced the following week in the more specialized editions of the journal. It has become clear that these services are valued by many readers and it has therefore been decided that this section of the journal will in future be enlarged and that the corresponding sections of *Nature New Biology* and *Nature Physical Science* will be reduced.

In the circumstances, however, it is still more important that readers should understand the way in which this section of the journal is compiled. In the first place, decisions about the developments to be reviewed in News and Views are made by the staff of *Nature* in consultation with what is now a small army of correspondents dealing with particular fields of science. Inevitably, there will be occasions when these decisions appear arbitrary—no one group of people, however large, could hope to cast a judicial eye over the whole of science and choose accurately and consistently all new developments of importance. The throng of correspondents is itself a varied group, including distinguished scientists with established reputations and much younger people who are, for one reason or another, committed to intensive and systematic reading of the literature in their chosen fields and willing and able to write about new developments in an interesting and even entertaining way. Those who write regularly have in the past been known as "Our Molecular Biology Correspondent" or some other such title, and great care is taken to ensure that there is a 1:1 correspondence between these contributors and their titles. In

future, such correspondents will be invited to decide for themselves whether their contributions are to be marked with their title or their initials. (This will not apply to the brief summaries of articles due to appear the following week in the Monday and Wednesday editions.) This, it is hoped, will serve to emphasize that these contributions to *Nature* cannot be and are not intended to be endowed with the majesty of permanent appraisals either of the merit or of the importance of some new development. Their function is frankly journalistic—to inform and, where possible, to stimulate.

An important problem in writing of this kind is the difficulty of being fair. In a necessarily brief comment on some scientific article, where it is necessary to say what the development consists of and possibly to link it with related developments in the field, it may be impossible fully to review the literature from which the new development has sprung and, given the variations there are bound to be in the way in which busy correspondents get their hands on the latest issues of the scientific journals, to take full account of recently published literature which may be relevant.

It is to be hoped that readers will be tolerant of deficiencies of this kind, which would of course be unforgivable in the formal scientific literature but which are permissible within the narrower framework of what is here intended—to inform and sometimes to stimulate. But with the hope of making some articles of this kind more comprehensive as reviews, it is planned that the longer comments on papers considered to be important will in future be tied less closely than in the past to the appearance of an article in *Nature*, *Nature Physical Science* or *Nature New Biology*, and that they will be still longer.

Primate C-type Viruses

IF last week's issue of *Nature* (235, 32; 1972) Spiegelman's group reported molecular hybridization experiments which indicate that human breast cancer cells but not normal breast cells contain RNA molecules with base sequences very similar if not identical to at least part of the RNA genome of mouse mammary tumour virus. The observations of these authors, of course, lend weight to the idea that the RNA virus recently discovered in the milk of some women, which closely resembles in structure and other diagnostic biochemical and biophysical parameters Bittner mouse mammary tumour virus, may be a human breast cancer virus. And if human milk virus is closely related to mouse mammary tumour virus, as seems to be the case, it is not unreasonable to think that human sarcoma and leukaemia viruses, equally closely related to the well characterized mouse leukaemia and sarcoma viruses, probably exist and await detection and isolation.

Indeed Spiegelman and his colleagues, as they mention briefly in their report, have pursued this line of argument and have apparently already shown that samples of several human sarcomas and human leukaemic tissue

contain RNA molecules homologous to the RNA in the Rauscher strain of mouse leukaemia virus. Such findings surely portend the discovery of human C-type RNA viruses, which will prove to possess the key taxonomic characters of the sarcoma and leukaemia viruses of lower mammals. It is indeed conceivable that the RNA C-type virus, some properties of which have been described by McAllister and his colleagues and Scolnick, Parks, Todaro and Aaronson in the last two issues of *Nature New Biology* (235, 3 and 35; 1972), is the first example of these putative human viruses.

The virus in question, currently known as RD114 virus, has a curious history. More than a year ago McAllister and his associates established in culture a line of human rhabdomyosarcoma cells (RD cells) in which they could find no evidence of the production of any RNA virus. Subsequently, they inoculated some of these RD cells into foetal kittens four of which developed disseminated sarcomas. Cells of two of these tumours, and a cell line (RD114 cells) established in culture from one of the tumours, were found to be producing a C-type RNA virus. The producer cells have a human chromosome complement, or karyotype, and are presumably the progeny of the initial human cell inoculum, but what is the origin of the virus they produce?

The kitten, from the brain tumour of which the RD114 tumour cell line was selected, was infected with feline leukaemia virus; one obvious possibility therefore is that the RD114 virus is a feline virus which was present in the kitten before it was inoculated with RD cells and which simply productively infected the human cells. The fact that feline leukaemia viruses infect human cells and the cells of many other species as well as those of its natural host, the cat, makes this suggestion very plausible. On the other hand there is always the possibility that the RD114 virus is of human origin, and that it was latent in the inoculated RD cells and induced to replicate during passage in the kittens.

In *Nature New Biology* McAllister and his many colleagues reported their attempts to decide between these alternatives. The sarcoma and leukaemia viruses of the lower mammals are classified into species specific groups chiefly by virtue of their host range, and most importantly by their antigenicity. The cat, rat, hamster and mouse sarcoma and leukaemia viruses contain internal proteins which are antigenic and group or species specific. For example, all the feline C-type viruses have a group specific antigenicity (gs 1) in common which is not present in the corresponding groups of mouse, rat or hamster viruses, each of which has its own unique gs 1 group specific antigenicity. Furthermore, however, all the lower mammalian sarcoma and leukaemia viruses share a second antigenicity, the so-called gs 3 or interspecies antigenicity.

The RD114 virus, if it is a typical feline C-type virus, should, therefore, contain the feline gs 1 antigenicity and the gs 3 antigenicity. If, on the other hand, this virus is the first of a new group, the human C-type viruses, it should contain gs 3 antigenicity but neither feline gs 1 antigenicity nor the gs 1 antigenicity of any of the other groups of mammalian sarcoma and leukaemia viruses. When McAllister *et al.* tested by various methods for these antigens they found that RD114 virus particles contain gs 3 antigen but not any of the known gs 1 antigenicities. Moreover, cat cells infected with RD114 virus remain fully susceptible to infection by feline sarcoma virus

which indicates that the antigens of the envelope of RD114 virus differ from those of the envelopes of the known feline C-type viruses.

RD114 virus is not, therefore, a typical feline leukaemia or sarcoma virus; it might be simply a feline virus which, for one reason or another, lacks detectable feline gs 1 antigenicity, or there remains the possibility that it is of human origin. Proof of its human origin will ultimately depend on showing that RD114 virus has a group specific antigenicity also present in other viruses of undisputed human origin and in human sarcoma and leukaemic tissues. All that has yet to be done but what Scolnick and his colleagues had to report (*Nature New Biology*, 235, 35; 1972) about the reverse transcriptase enzyme in RD114 virus and two other C-type viruses from lower primates certainly does nothing to diminish the possibility that RD114 virus is of human origin.

Scolnick *et al.* have investigated the immunochemical and other properties of the reverse transcriptases of four viruses that are produced by lines of cultivated primate cells. The viruses are the RD114 virus, the virus liberated by the human ESP-1 cell line (Priori *et al.*, *Nature*, 232, 61; 1971), which is almost certainly a contaminating murine virus (Gilden *et al.*, *Nature*, 233, 102; 1971); and two viruses, one from a line of woolly monkey fibrosarcoma cells, and the other from a line of gibbon ape lymphoma cells.

All these viruses contain the gs 3 interspecies specific antigenicity. They all also contain reverse transcriptases which, by virtue of their responses to different synthetic DNA/RNA hybrid templates, can, Scolnick *et al.* believe, be classified with the reverse transcriptases of lower mammalian C-type viruses and other enveloped viruses and distinguished from cellular DNA polymerases. Moreover, the two monkey cell viruses can provide the helper function required by defective mouse sarcoma genomes for their replication in rat cells and in this respect the two monkey viruses behave like the leukaemia viruses of the lower mammals.

As Todaro's group showed last year, although the reverse transcriptases in the groups of lower mammalian sarcoma and leukaemia viruses are very similar enzymes they are not immunochemically identical. For example, antibody against the murine enzyme inhibits murine reverse transcriptase to a greater extent than it inhibits the enzyme in feline or hamster viruses, and the converse is equally true. Scolnick and his colleagues therefore measured the extent to which the reverse transcriptases from their four putative primate C-type viruses were inhibited by antisera against the murine and feline enzymes. The enzymes from RD114 virus and from woolly monkey and gibbon ape viruses were not inhibited appreciably by either antiserum. By contrast, the reverse transcriptase from ESP-1 virus was strongly inhibited by antimurine reverse transcriptase antiserum. This latter finding confirms that the ESP-1 virus is of murine origin, a contaminant. On the other hand, the fact that the reverse transcriptases of the two monkey cell viruses and the RD114 virus are not inhibited by these two antisera suggests that these three enzymes are immunochemically distinct from their counterparts in the lower mammalian sarcoma and leukaemia viruses. As Scolnick *et al.* suggest, it may well be, therefore, that these three viruses belong to a new and immunochemically distinct group, the primate C-type viruses.

Two Families of X-ray Variables

ONE of the most important developments in astronomy in 1971 was the discovery, both by satellite and by rocket-borne detectors, of several pulsating X-ray sources. Previously, only NP0532 in the Crab was known as an X-ray pulsar; this caused some embarrassment among high energy astrophysicists, because theory predicted that other sources of the same kind should also exist. The discovery of other pulsating X-ray sources did not immediately alleviate this embarrassment, however, because they did not neatly fit into the preconceived categories. It now seems, however, from the work reported by G. R. Blumenthal and W. H. Tucker on page 97 of this issue of *Nature*, that one class of X-ray pulsation—that typified by the activity of Cyg X-1 and Cir X-1—might be explained in terms of oscillations associated with a high magnetic field and produced by giant flare-like events.

The first report of periodic variations in the X-ray flux from Cyg X-1 came from R. Giacconi, describing observations carried out by the Explorer 42 satellite. These showed a variation at fifteen pulses a second, a figure which really put the cat among the pigeons because of the lack of any similar pulses at optical or radio frequencies. Pulsar theories now regarded as conventional could not explain this discrepancy. If a pulsar were young enough to be rotating fifteen times a second and to be producing X-ray pulses, it should be producing optical and radio emission, and be surrounded by the debris left by the supernova in which it formed. In spite of excited talk of black holes, the suspicion remained that pulsar theories might have to be completely revised.

In the light of further observations, however, it became clear that the problem is more concerned with the diversity of natural phenomena. The rotating neutron star model of pulsars is probably fine as far as it goes; the mistake lay in believing that one simple model could describe all pulsars and related phenomena. Only a few weeks after Giacconi's announcement, S. Holt *et al.* produced evidence pointing to a much more complicated structure in the fluctuations of Cyg X-1, and in the second part of 1971 the picture which has emerged is that of a source producing

large fluctuations of intensity on time scales of 50 ms to 10 s or longer, in which individual pulses can at times account for half of the observed power. Pulse trains with periods of 0.3 to 10 s, each lasting for up to several tens of seconds, are also observed, and these contain 10 to 25 per cent of the observed power. But no single period is consistently observed. In this respect, the X-ray fluctuations of Cyg X-1 show a superficial similarity to the optical fluctuations of Sco X-1, which may be associated with optical flares. In 1971, H. Kestenbaum *et al.* compared rocket X-ray data with simultaneous optical observations by W. J. Cocke and found that optical fluctuations of this kind were also associated with X-ray fluctuations, although on this occasion they did not find evidence for any preceding flare, perhaps because of the incompleteness of the observations (*Astrophys. J. Lett.*, **169**, L49; 1971).

At present, then, it seems that there

are at least two classes of X-ray star which produce radiation containing periodic structure. First, there is the conventional pulsar, in which some kind of "whole star" effect produces extremely regular and continuous pulses. Second, there is the Sco X-1 and Cyg X-1 family in which local effects in the star's magnetic field and atmosphere may produce short-lived periodicities. The edges of this classification may, of course, be somewhat blurred, and there may be other categories. But probably most kinds of X-ray star can be explained in terms of some combination of the properties of these two archetypes.

Blumenthal and Tucker offer an interesting explanation of the second category of X-ray periodic variables. Pulsations in trains lasting for a few minutes with periods of a few seconds have been seen in solar flares, and they suggest that a "ringing" of the magnetic field structure could explain both this effect and the periodicities seen in Cyg X-1.

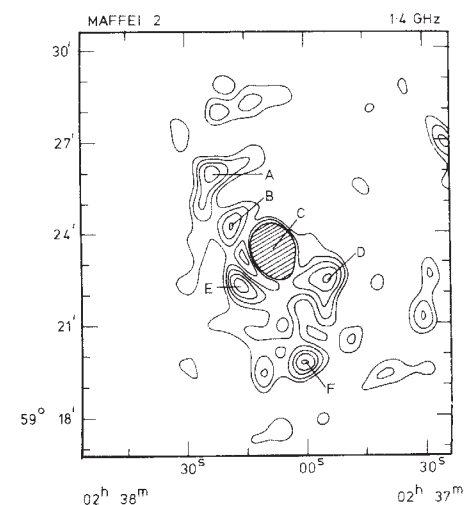
Maffei 2, an Sb Spiral Galaxy

Two partly obscured objects reported by P. Maffei in 1968 have been attracting a deal of attention in the past year as possible undiscovered galaxies near the fringe of the Local Group of galaxies. In next Monday's *Nature Physical Science* (January 17), R. Love of the Cambridge radio astronomy group describes detailed radio mapping of Maffei 2 with the half-mile interferometer. The illustration reproduced here shows the continuous emission from Maffei 2 at 1421.6 MHz, and several distinct components can be recognized: a central core C and several lesser centres of emission.

From this work, which also includes an examination of the appearance of Maffei 2 in the 21 cm emission from neutral hydrogen, Love is able to deduce several important parameters. For example, Maffei 2 appears to be an Sb galaxy (a class of spiral galaxies to which the Milky Way galaxy probably belongs), the rotation axis of which is inclined to the line of sight by an angle of probably about 55°, and which is at a distance of 4 Mpc. This makes the linear diameter of the core about 680 pc.

It is interesting that Love finds no evidence of distortion of the distribution of neutral hydrogen in Maffei 2 that can be attributed to Maffei 1, which is probably an elliptical galaxy of type E3 or E4. The two Maffei objects seem to be unrelated.

Finally, the 4 Mpc distance of Maffei 2 places it beyond the Local Group of galaxies, and, as others have done, Love suggests that it is in the Ursa Major Camelopardalis cloud of galaxies. A group at the Observatoire de Meudon who have estimated a distance of 2.7 Mpc for Maffei 2 have placed it in the M81 subgroup of the cloud; Love, having estimated a distance of 4 Mpc, thinks it more likely that Maffei 2 is in an obscured subgroup of the cloud which also contains IC 342, IC 356, NGC 1560 and NGC 1569.



A map of the continuous emission from Maffei 2.

CRICKETS

Genetics of Call

from our Neurophysiology Correspondent

MALE crickets make highly standardized calling songs which they can develop in isolation, without hearing a model. Each call consists of a chirp containing four to six sound pulses, followed by a series of trills containing up to fourteen sound pulses. The call patterns depend on activity in small groups of neurones within the thoracic ganglia; these drive fast muscles each of which contains no more than three motor units. Muscular activity produces stridulation by opening and closing the wings. The neural networks produce the song following an endogenous rhythm that can be entrained by the normal light-dark cycle. Such stereotyped behaviour patterns, under the control of small networks of identifiable neurones, are ideal subjects for the investigation of the mechanisms underlying the formation of neuronal connexions and its translation into behaviour.

D. R. Bentley (*Science*, **174**, 1139; 1971) has crossed two species of cricket with very different calls, *Teleogryllus commodus* and *T. oceanicus*, and has then made back-crosses between the male F_1 hybrids and their parents. (The female F_1 hybrids were sterile.) He recorded the calling songs of the parents and all the crosses and investigated various of their characteristics including the number of trills within a call, the number of sound pulses within a trill and the intertrill interval. He also recorded the activity of the wing opener and closer muscles, the action potentials of which are produced by the discharge of a single neurone. The pattern of the bursts of action potentials from the muscles determines the pattern of sound pulses, and the number of action potentials within each burst determines the amplitude of the sound pulses.

Bentley's results show that there are at least several genes involved in determining the sound pattern of a call and that these are on more than one chromosome because some call characters are sex-linked whereas others are not. The genetic control may be very precise, for in one instance there was a genetically determined difference of a single action potential in the bursts from single, homologous, neurones of different genotypes. Indeed, it might be expected that the developmental control system specifying the parameters of a calling song would be both polygenic and capable of very precise action. Presumably the features of a song have evolved in a more or less continuous fashion; this process can occur as the result of changes in activity of several muscles as well as in the structure of various skeletal elements. Selection

leading to the divergence of species-specific song patterns could therefore have acted at many independent loci. This is obviously likely to be the case for any piece of behaviour, however simple, depending as it must on the interaction of many structures of different developmental origins.

BIOPOLYMERS

Make and Break

from our Molecular Biology Correspondent

THERE are no doubt people—molecular biologists even—who will receive the news that the kinetics of oligonucleotide pairing have been measured and analysed with an equanimity bordering on indifference. Nevertheless, for anybody with a serious interest in such processes as codon-anticodon interaction and unwinding of DNA, two new articles covering essentially identical ground will re-

pay close study. They concern the equilibrium between oligomers of adenylic acid and uridylic acid, and their two-stranded complexes. Much work has in the past been done on the kinetics of melting of DNA, but this is a problem of hideous complexity. In the first place there are two kinds of base pair, with presumably different kinetic characteristics; in denaturation, rotational friction effects become important at appreciable molecular weights, whereas in renaturation intrastrand interactions, multiple nucleation and other unsought-for phenomena manifest themselves. There is consequently much to be said in favour of starting from the relatively tractable situation presented by simple oligomers.

Craig, Crothers and Doty (*J. Mol. Biol.*, **62**, 383; 1971) have used the temperature-jump method to examine the interaction of oligomers of chain lengths from 4 to 7, whereas Pörschke and Eigen (*ibid.*, 361) have worked with a series of

A "Non-internalizable" Mitogen for Lymphocytes

THERE is little agreement as to the general nature of regulators of mitosis among somatic cells; contact inhibition, hormones, chalones, and quasi immunological processes number among the very many controls which have been proposed. The technical difficulties of establishing exactly what is happening in a solid block of tissue are in fact formidable and it may be many years before these problems can be fully resolved. In the interim the lymphoid system and lymphocytes in particular provide an interesting analytical tool. Mitoses are part of the reaction pattern of the lymphoid system to antigenic stimulation and lymphocytes can be stimulated to mitotic activity *in vitro* in a number of different ways. In next Wednesday's *Nature New Biology* (January 19), Greaves and Bauminger set out to explore this last phenomenon and to point the way to further understanding of the manner in which somatic cells can be made to divide.

Greaves and Bauminger have covalently bound the well known plant mitogen, PHA, to 'Sephacrose' beads. These beads are insoluble cross linked polysaccharide aggregates the surface of which can be activated by cyanogen bromide in such a manner that covalent binding of proteinaceous material is possible. The ability of the 'Sephacrose'-PHA conjugates to induce transformation and DNA synthesis (and presumably mitosis) in populations of B and T lymphocytes was studied. B cells were obtained from the spleens of thymectomized irradiated mice which had been given haematopoietic tissue therapy consisting of anti- θ treated syngeneic bone-marrow (to eliminate T

cells in the marrow), and T cells were collected from the thymuses of cortisone treated mice. It is accepted that cortisone-resistant cell populations in cortisone-treated mice are roughly equivalent to true thymus derived cells collected outside the thymus. It was found that both T and B cells responded to PHA on the surface of 'Sephacrose' beads.

This result is of interest in a number of ways. First, the capacity to respond to PHA has been used as an identifying characteristic of T cells. It is now shown that if PHA is presented to B cells in an appropriate manner they will also yield to its persuasive urge to reproduce. Second, and more important, it suggests that a cell can be made to divide by surface stimulation as Greaves and Bauminger believe that the PHA is retained on the 'Sephacrose' bead surface—in their quaint terminology, it is non-internalizable. Evidence is indeed presented that less than 2 per cent of the bound PHA leaches off the bead and that this is insufficient to account for the effect observed. The unlikely possibility that polysaccharide fragments from the bead itself are responsible is disregarded.

Greaves and Bauminger suggest that stimulation of lymphocytes by PHA is comparable with their activation by other antigenic materials and they point to analogies with stimulation of cells *in vitro* by covalently bound peptide hormones. Without precluding the possibility that pinocytosed material can be of significance in relation to cell division, the finding of Greaves and Bauminger could well establish the important principle that the initiation of division of a somatic cell takes place at the cell surface.

chain lengths between 8 and 18, and have happily arrived at similar conclusions. Within the right range of temperature and ionic strength the intrusion of three-stranded helices can be avoided. There are then two processes in the formation of complementary dimers, namely the association of some small number of bases to nucleate the helix, and the formation of subsequent base pairs starting at these nuclei. The first of these is entropically expensive, does not have the full benefit of stacking interactions, and is thus associated with a much lower equilibrium constant. For the short-chain molecules, the kinetics can, it transpires, be substantially accounted for by a single bimolecular rate process, which can only be interpreted as reflecting an essentially all-or-none interaction, all, or nearly all, the base pairs being made or broken at once. The relaxation times give the forward and reverse rate constants, and the former decreases with rising temperature, showing that it must represent, as expected, a combination of two steps, one of them rate determining. The rate constant for the reverse (melting) reaction depends markedly on chain length.

Craig *et al.* then go on to estimate the number of base pairs involved in the nucleation step from the activation enthalpy for the overall forward rate process, using published calorimetric data for the enthalpy of formation of an A-U base pair. The answer comes out at 2-3 base pairs for the size of the nucleus. Thus one envisages a rapid association-dissociation equilibrium involving the adhesion of the two chains by two or three base pairs. When these are in proper register the complete double-helix forms like the closure of a zip fastener. Because the concentration of nuclei governs the rate of the association, the reaction is second order. The zipping-up process is not observed, because states of intermediate pairing are not significantly represented (except for a small contribution from duplexes with frayed ends).

The relation between dissociation rate and chain length is conversely explained by the dependence of the process on the number of base pairs that must break before the duplex is whittled down to the critical nucleus length, at which the chains rapidly separate. For these very short chains the problem of annealing by creeping of one strand relative to the other, so as to maximize the base pairing, does not arise.

Pörschke and Eigen are in agreement with Craig *et al.* at all essential points. Arguing again from the value of the activation enthalpy for the association reaction, they conclude that a nucleus of three base pairs must be formed before the dimer can survive. This Pörschke and Eigen view as implying

two pre-equilibrium steps, for if a complex with two base pairs is unstable, it can make and break repeatedly before the third pair is added. On this reckoning it is the formation of the third pair which is rate determining. Long chains show markedly different behaviour from short: they display a spectrum of relaxation times, reflecting melting of imperfect helical species. Association rate constants are very high, indicating that nucleation occurs at multiple points along the chain. The stability of the triplet as a minimum associative unit is consistent with biochemical experience, and Pörschke and Eigen speculate that the marginal stability of triplet codons may be important for rapid scanning of a nucleic acid sequence.

The implication of a rapid nucleation followed by a slow rearrangement has been found in a quite different system by Tsong, Baldwin and Elson (*Proc. US Nat. Acad. Sci.*, **68**, 2712; 1971). In the thermal denaturation of ribonuclease they have succeeded by the temperature-

jump method in detecting the presence of a process in the millisecond time range, several orders of magnitude more rapid than the previously observed slow conformation change.

PALAEOGEOGRAPHY

Drift and Distribution

from a Correspondent

A JOINT symposium of the Geological Society of London, the Palaeontological Association and the Systematics Association was held in Cambridge on December 15 to 17 to discuss the topic of organisms and continents through time. The occasion proved an excellent opportunity for geophysicists and palaeontologists to stage a combined assault on the many problems surrounding the relationships of continental drift, palaeoclimates and faunal and floral distributions during Phanerozoic time. The basis of the meeting was a set of world

Neutral Hydrogen near the Sun

KNOWLEDGE about the immediate neighbourhood of the Sun is steadily improving, with the help of both optical and radio-astronomical observations. Through 21 cm observations, for example, data concerning the hydrogen distribution in the galaxy are by now quite extensive. Macchetto and Panagia, of the Frascati Space Astrophysics Laboratory, recently suggested that the study of Lyman- α absorption in the spectra of suitably chosen stars could help to determine the mean volume density of neutral hydrogen. Unfortunately there are rather stringent requirements to be met, and the determinations of the neutral hydrogen density by this method are still only in their infancy. The reasons are, first, that one measures the absorption of a line which only appears in the spectrum of an early type main sequence or a horizontal branch star, and that one must be sure that the emission in fact comes from the star and not from a circumstellar envelope. Second, it is necessary to know the distance of the star in question, and, third, the basic atomic physics data for the absorption are relatively uncertain.

In an article in next Monday's *Nature Physical Science* (January 17), Macchetto and Panagia present the first data determined by this method. They find a density of about one hydrogen atom per 4 cm^3 from the Lyman- α absorption data of four stars, one O-star and three B-stars, at estimated distances of between 80 and 500 pc. The authors feel that their result is meaningful, although they stress the preliminary nature of

their conclusions. It should be emphasized that the density quoted is the one in the immediate vicinity of the Sun, and that the fall-off of the density with galactic latitude has been duly taken into account.

Perhaps the most interesting part of Macchetto and Panagia's analysis is their comparison with 21 cm emission data in the same directions as the lines of sight to the four stars. In that case, the integration is performed over an infinite path length, rather than over the finite path length which terminates at the star. For α Vir, a B-star in a direction well away from the galactic plane, the hydrogen densities emerging from Lyman- α absorption data and from 21 cm emission data agree well. The agreement seems to be rather poor, however, for the two stars in Orion (σ Ori, an O-star, and η Ori, a B-star); the 21 cm data lead to a density of about 1 cm^{-3} , which is about four times larger than the density suggested by the Lyman- α data. But there are indications of the existence of a cloud of hydrogen in the Orion region, and Macchetto and Panagia show that it is likely that this cloud leads to a value of the 21 cm emission which is about four times the value it would have in the absence of the cloud, and that, moreover, this cloud is likely to lie behind the stars. Tentatively, therefore, Macchetto and Panagia come to the conclusion that 21 cm emission data and Lyman- α absorption data lead to the same value for the neutral hydrogen density in the immediate vicinity of the Sun.

maps compiled by Dr J. C. Briden (University of Leeds), Dr A. G. Smith and G. E. Drewry (University of Cambridge) and distributed to contributors before the meeting. These maps showed the inferred latitude and longitude of the present continents at various intervals during the Phanerozoic.

In introducing the maps, Drs Briden and Smith explained that they had been compiled from published information on seafloor spreading, geometric fit and palaeomagnetic data. It had been possible to use this information with some confidence to wind up the oceans back to the Permian and create a "Pangea". For earlier periods, however, it had been necessary to break up the existing continents. The criteria for doing this were obscure and the palaeogeography was thus more speculative.

Methods of study of biological distribution were discussed by five contributors. Professor P. H. A. Sneath (University of Leicester) examined how far the degree of contiguity of land areas might be assessed from the dissimilarity of their biotas. Apart from physical and climatic barriers to migration, the timing of the radiative evolution of a taxon relative to continental drifting, and its mobility since then, affected its present distribution. An analysis of the dissimilarity of genera of conifer and notostracan crustacea correlated well with the present distribution although the short biotic distance between the conifers of the southern hemisphere was a relic of past palaeogeography.

Professor F. J. Vine (University of East Anglia) presented a new method for pole prediction from a limited amount of latitude dependent data by fitting a fourth degree polynomial for points on a global grid of trial "north poles". A confidence area for the predicted pole was then contoured from the grid. This method accurately predicted the present north pole from present faunal distribution. When applied to data for Permian brachiopods plotted on the present world map it gave several "north poles". The brachiopod data and also Permian palaeomagnetic data plotted on the Briden-Smith Permian map predicted a "north pole" about 20° along the 0° meridian suggesting that the map may need modification.

Dr A. Hallam (University of Oxford) analysed some patterns of faunal distribution through time and showed how these may relate to plate tectonics. Convergence in degree of resemblance between two land faunas (for example, Africa and Eurasia) may be complemented by the divergence of adjacent marine faunas (Mediterranean and Indo-Pacific). Disjunct endemism was illustrated by late Palaeozoic fossils on the present southern continents and by the Lower Jurassic bivalve *Weyla*, now

fossil in western North America, Mediterranean and East Africa.

The distribution of Mesozoic and Tertiary organisms was next discussed. The history of African mammals was reviewed by Drs R. J. G. Savage and S. G. Coryndon (University of Bristol). The mid Tertiary collision of Africa and Eurasia was clearly correlated with the spread of previously African endemics, notably proboscideans and sirenians, and the enrichment of African faunas from Asian stocks. Dr F. C. Dille (BP, Sunbury-on-Thames) showed how the essentially tethyan larger foraminifera repeatedly spread to central and western America, Madagascar and Australasia in spite of ocean spreading. On the other hand Dr A. G. Coates (George Washington University) described the increasing provinciality of hermatypic corals, nerineid gastropods and rudist bivalves in the Caribbean, Mediterranean and Middle East which could be correlated with the opening of the central Atlantic and the closing of Tethys. Mesozoic floras plotted by Dr P. D. W. Barnard (University of Reading) on the Briden-Smith maps seemed to be dependent on latitude although for the Lower Triassic there was no evidence for any floras within the tropics. Mr N. F. Hughes (University of Cambridge) considered that the distribution of conifers, early angiosperms and the dispersed spores *Classopollis* and *Normapollis* on the relevant Briden-Smith maps showed some dependence on latitude and could be useful in assessing past climates.

Finally, Palaeozoic distributions were considered in relation to the more speculative Palaeozoic maps. Professor H. B. Whittington and Dr C. P. Hughes (University of Cambridge) and Professor A. Williams (Queen's University, Belfast), in discussing Ordovician trilobites and brachiopods respectively, ascribed decreasing provinciality (highlighted in mid-Caradocian times) to the closing of the Proto-Atlantic. Dr W. G. Chaloner (University College, London) contrasted the uniformity of the Lower Carboniferous flora with the Euamerican, Angara, Cathaysia and Gondwana floras developed by the Lower Permian. These floral regions showed some dependence on latitude when plotted on the Briden-Smith map but the boundary between the Angara and Cathaysia floras lay oblique to latitude. Low provinciality was apparent in the Silurian although Drs L. R. M. Cocks (British Museum (Natural History)) and W. S. Mckerrow (University of Oxford) were able to distinguish a South American-West African *Clarkeia* fauna and, in the late Silurian, a Nearctic-north Asian *Atrypella* fauna which suggested the close proximity of these areas. The distribution of Devonian goniatites (Professor M. R. House, University of Hull) showed a generalized tropical distribution on the Briden-Smith map but suggested that Africa and eastern North America were adjacent. Carboniferous goniatites (Dr F. Hodson, University of Southampton), although cosmopolitan, also showed a broad tropical distribution except for those of east Asia.

Insulin Release in Laboratory Diabetes

THERE are now known to be a very wide variety of syndromes involving experimental diabetes in animals. Some of these are relatively bizarre in type, such as the diabetes induced by a change of diet in the sand rat, but most often the type of diabetes in such syndromes is marked by obesity and relative mildness. In but a few of these diseases has the defect been adequately characterized, and the immediate relevance of these conditions to human diabetes is far from clear.

In next Wednesday's *Nature New Biology* (January 19), Larkins and Martin report their observations on the responsiveness of beta cells in the islets of Langerhans of New Zealand obese mice to various agents. Neither glucose, the sulphonylurea tolbutamide nor glucagon were effective in promoting insulin release from the beta cells in such animals. By contrast, arginine was a very potent agent for insulin release—much more so than in control mice. Larkins and Martin suggest that the inability of these animals to respond to glucose is responsible for their diabetic state, and

that the hyperinsulinism which characterizes them is presumably related to effects of amino-acids, perhaps derived from the diet, on the beta cell. The authors also comment that a similar situation may exist in the foetal islet which is known to be much more responsive to amino-acids than to glucose.

The significance of these observations for a fuller understanding of the origin of human diabetes is, however, much less apparent. Thus, although a defect in the mechanism of insulin secretion has been incriminated in the poor insulin response of some close relatives of diabetics, its importance in the development of the fully manifest disease is very obscure, as has been emphasized by Pyke and his colleagues (*Brit. Med. J.*, 4, 694; 1970). Moreover, many cases of diabetes are absolutely deficient in insulin from the outset. Nevertheless, the results of Larkins and Martin are interesting enough to suggest that further metabolic studies of a detailed kind should be undertaken on the beta cells in other types of laboratory diabetes.

GRAVITATION

Waves from Black Holes

by our Cosmology Correspondent

ACCORDING to the conventional understanding of relativity, collapsing black holes should produce gravitational radiation in the form of pulses, like those detected by Weber. But under some circumstances it seems that a black hole can produce long trains of gravitational waves, and it may be that the bursts of radiation detected by Weber arise from this process. The important feature of a disturbance which will produce such trains of radiation is that it perturbs a black hole from its preferred state of spherical symmetry (W. H. Press, *Astrophys. J. Lett.*, **170**, L105; 1971). It is no surprise that such a perturbed black hole will radiate gravitational waves as it restores sphericity; but it is surprising to learn that this radiation may be emitted in a long train of nearly sinusoidal waves, rather than in a single intense burst.

Following calculations of perturbations of integer-spin zero rest mass fields by R. H. Price, Press has built on earlier work of Regge and Wheeler (*Phys. Rev.*, **108**, 1063; 1957) and of Zerilli (*Phys. Rev.*, **D2**, 2141; 1970) which describes the dynamics of the process for the odd parity and even parity cases. It seems that a crude physical picture of the situation can be gained by imagining the black hole to vibrate around spherical symmetry in a quasi-normal mode which is slowly damped by gravitational radiation.

But what excites these oscillations? Somehow, the black hole must be struck, to set it ringing like a bell. There is reason to believe that a test particle falling into a black hole can excite vibrational modes (Davis *et al.*, *Phys. Rev. Lett.*, **27**, 1466; 1971); in order to produce the long wave trains predicted by Press disturbances with a high spherical harmonic index (multipolarity $\gg 1$) must be excited. This raises some difficulties with the excitation mechanism, but in real systems quite complex vibrations might be caused when the hole is accreting from, say, a surrounding ring of matter.

The most intriguing feature of this work is the discovery that black holes might be able to interact with the rest of the universe in a quite complex way. If wave trains of gravitational radiation are discovered, or if the Weber pulses turn out to have detailed structure of the kind postulated by Press, there is, in principle at least, a tool available for studying the properties of black holes such as their rotation which will surely affect the radiation. And because high multipole vibration can produce relatively short-wavelength radiation even from massive objects, this mechanism

sets no upper limit on the mass of a black hole which might produce waves suitable for detection by Weber's apparatus. This could explain the apparently excessive number of gravitational wave events reported by Weber in recent years, which has seemed too large to account for simply in terms of the collapse of supernova remnants to form black holes.

LIPID MEMBRANES

Carriers and Pores

from our Membrane Correspondent

By incorporating various antibiotics, bilayer lipid membranes (BLM) can be made to mimic some of the ion permeability properties of natural membranes and this model system has been extensively studied in recent years. There are essentially two possible mechanisms by which the antibiotics can transport ions across these membranes—either by acting as mobile carriers which diffuse back and forth across the membrane or by forming pores which offer selective pathways for ion movement. Which of these two mechanisms is the one which is operative has been the subject of some controversy but the majority view

seems to be that macrocyclic antibiotics of relatively small size, such as nonactin and valinomycin, act as carriers, whereas more complex antibiotics such as gramicidin A and alamethicin form pores. This distinction has recently been put on a firmer footing by the work of Krasne, Eisenman and Szabo (*Science*, **174**, 412; 1971).

Krasne *et al.* have examined the effect of nonactin, valinomycin and gramicidin on BLM above and below the thermal transition temperature of the lipid constituting the membranes. This temperature is that at which, on cooling, the state of the long hydrocarbon chains of the lipid molecules changes from a relatively fluid state to a fairly rigid array similar to that in solid hydrocarbons. Krasne *et al.* found that the permeability effects (as judged by membrane conductance) of nonactin and valinomycin were abolished by cooling the membrane through its transition temperature whereas the effect of gramicidin was only slightly reduced. These antibiotic molecules are too large to fit into the interstices between the lipid hydrocarbon chains and so in order for them to act as mobile carriers it is necessary for the membrane to be in its fluid state.

Doing without Tyrosine Residues

In next Wednesday's *Nature New Biology* (January 19), Hayashi and Stamatayonopoulos present an examination of the properties of two abnormal human haemoglobins, which correlate satisfyingly with Perutz's model for the oxygenation mechanism. The two mutant forms, haemoglobins Rainer and Bethesda, are of particular interest in being the only known variants with substitutions for the important tyrosine residues next to the termini of the β -chains. Perutz has shown that in normal haemoglobin, in the deoxygenated state, these side phenolic groups lie in pockets, making hydrogen bonds with backbone carbonyls. On oxygenation, the tyrosine side chains are displaced.

Just like normal haemoglobin deprived of these tyrosines by carboxypeptidase treatment, haemoglobins Rainer and Bethesda show anomalously high oxygen affinity and only vestigial haem-haem interactions, as well as a reduced Bohr effect. The natural cofactor, diphosphoglycerate, is also shown to have little effect on the oxygenation equilibrium of the variants. Moreover, in haemoglobin Bethesda, which (unlike haemoglobin Rainer) possesses the normal complement of sulphhydryl groups, the chemical reactivity of the latter is quite different from those of normal haemoglobin; not only do they react unusually rapidly in the oxygenated form, but, by contrast with

normal haemoglobin, the sulphhydryl groups of which become very unreactive on deoxygenation, those of haemoglobin Bethesda actually react much more rapidly after deoxygenation.

All these observations are readily explicable in terms of Perutz's scheme. There seems to be no question of any inability to enter the characteristic deoxyhaemoglobin conformation. Indeed, Greer and Perutz have shown that deoxyhaemoglobin Rainer, as well as carboxypeptidase treated normal deoxyhaemoglobin, are both in that state in the crystal. Rather it seems that the free energy difference between the deoxyhaemoglobin and oxyhaemoglobin conformations is greatly lowered, so that in the deoxygenated state a measurable part of the population has the oxyhaemoglobin conformation. This is a consequence of the absence of the stabilizing contribution of the tyrosines in the β -chain pockets.

Evidently the same tyrosines are also needed to create the cofactor binding site. In the absence of the vital tyrosines, a salt bridge, involving the adjoining histidine, is not made, and this interaction was postulated by Perutz to account for a large part of the Bohr effect. Furthermore, the salt bridge in deoxyhaemoglobin physically occludes a sulphhydryl group in each β -chain, so that the much higher thiol reactivity in haemoglobin Bethesda is also accounted for.

In the case of pore formation, it would be expected that once a pore has been formed, the change of state of the lipid hydrocarbon chains would be relatively unimportant unless it brought about a drastic change in antibiotic solubility. Thus one explanation of the results of Krasne *et al.* is that all three antibiotics form pores but on "freezing" the membrane the solubilities of non-actin and valinomycin are drastically reduced whereas that of gramicidin is unaffected. A simpler and more attractive interpretation, however, is that non-actin and valinomycin act as carriers whereas gramicidin forms pores.

It is generally accepted that gramicidin associates to form dimers in BLM and, early last year, Urry (*Proc. US Nat. Acad. Sci.*, **68**, 672; 1971) proposed that the conformation of the gramicidin molecules in this state was that of the unusual π (L,D) helix. More recently, Urry, Goodall, Glickson and Mayers (*ibid.*, **68**, 1907) have extended this work by chemically coupling the α -amino groups of two deformyl gramicidin A molecules and have examined the effect of the resultant malonamide dimer on BLM. Dimerization in the membrane seems to be an important step in the mechanism of action of gramicidin, but in the case of the malonamide dimer this step should be eliminated. The effect of the malonamide dimer on the kinetics of the development of membrane conductance, the form of the dependence of this conductance on lipid composition and the absence of autocatalytic behaviour are all consistent with this view. Urry *et al.* have thus obtained more evidence that gramicidin dimerizes in the membrane to form conducting pores. Whether in so doing it adopts the proposed π (L,D) helical structure is open to doubt, because Urry has not yet presented any convincing spectroscopic or other evidence to confirm this structure.

CHROMOSOMES

Drosophila dAT

from a Correspondent

ONE of the most challenging problems in cell biology at present is without doubt the role and behaviour of DNA with highly reiterated nucleotide sequences. Be they density satellites as in mouse, or of the same overall base composition as in rat, the very large amounts of this DNA in each cell signify its importance. There is no clear evidence that this DNA is ever transcribed, but several groups have shown that there is a strong association with the heterochromatic regions of the cell nucleus. In the mouse, where the satellite DNA represents about 10 per cent, or four chromo-

somes-worth of the nuclear DNA, Jones and others have shown by hybridization *in situ* that much of it is associated with the chromosomal centromeres. With the possible exception of the Y chromosome, each chromosome seems to carry some of the light satellite DNA.

Perhaps the most bizarre of all the reiterated DNAs is the family of dAT copolymers first found by Sueoka in *Cancer borealis* (*J. Mol. Biol.*, **3**, 31; 1961), where it comprises more than 30 per cent of the nuclear DNA. In this crab each strand of the DNA double helix is almost exclusively the alternating copolymer d(AT), whereas in *Drosophila melanogaster* 4 per cent of the nuclear DNA is the apposed copolymer dA.dT. Furthermore, *Drosophila* possesses incomparably better genetics. With this tool at hand Blumenfeld and Forrest have recently reported (*Proc. US Nat. Acad. Sci.*, **68**, 3145; 1971) an investigation of the chromosomal location of the *Drosophila* dAT.

Using several genetic tricks, Blumenfeld and Forrest have constructed a series of flies with extra Y chromosomes or extra pieces of the Y chromosome. They have analysed the proportion of dAT in each of their strains and have discovered that the more Y chromosome pieces they have, the higher the proportion of dAT satellite. Although the Y chromosome is not the sole genetic element involved because XO flies contain 2 per cent dAT, it is nevertheless striking that the dAT content of the fly can be increased by increasing the Y dosage. There are clearly two chief alternative explanations: the Y chromosome either carries a control element which governs the synthesis or survival of an extra dollop of dAT, or it itself is particularly rich in dAT.

These alternatives can obviously be distinguished by the use of hybridization *in situ*; if the second alternative is correct it will be another example of a reiterated sequence being located on a particular chromosome; if the first

Understanding the Hadron Era

THE big-bang theory of cosmology, which postulates that the universe exploded from an infinitely dense state according to the laws of general relativity, has been successful in predicting several features of the universe, notably its expansion, the relative abundance of elements and the black-body background radiation. A remarkable, yet unexplained, feature is the tendency of matter to coagulate in large clumps—not stars, the formation of which by mutual gravitational attraction of diffuse matter is understood, but galaxies.

At one time it was hoped that cosmological theories might promote the idea of galaxy formation in a way similar to star formation. If that had been the case, the evolution of the universe, through a state dense enough to make radiation the predominant contribution to pressure into the diffuse condition of the present, might have included an epoch during which small statistical fluctuations from completely homogeneous density had a tendency to grow, both in size and in degree of inhomogeneity, to galactic dimensions and densities. This hope has not been realized, and the existence of galaxies is often ascribed nowadays to primaeval conditions, to large non-statistical inhomogeneities from before the big-bang.

It is still important to check that there is no tendency for fluctuations to be damped out and to disappear. The article by Mészáros in next Monday's *Nature Physical Science* (January 17) shows that there is no such tendency during what is called the hadron era.

The hadron era is the time when the density of the universe was comparable with that of an atomic nucleus. This was a very short era and it ended when the universe was 10^{-1} seconds old. Such densities now exist on a large scale only in neutron stars.

A difficulty in understanding such conditions is that the equation of state is not known. In the early universe the equation of state is more complicated even than inside neutron stars, for the matter is hot: that is, the kinetic energy of the constituent particles is comparable to their rest masses. Constraints can, however, be put upon the equation of state by the requirements of causality; the velocity of sound in the material must not exceed that of light if cause is to precede effect for every observer. Moreover, there are records of the equation of state derived from nuclear physics.

Mészáros investigates the problem and disproves a conjecture that the pressure-volume relationship should be that of a cool monotonic gas, which had been suggested as a model stable against any tendency for statistical density fluctuations to collapse into an infinitely dense condensed state. Mészáros gives reasons for restricting the discussion to two equations of state; which should actually be used depends on whether or not the universe initially contained as many anti-particles as particles.

Mészáros uses these equations of state to investigate the fate of density fluctuations, and shows that galaxies cannot have evolved from statistical fluctuations during the hadron era.

is the case then there is the exciting prospect of a gene, perhaps responsible for specific amplifications, in an organism whose genetics are the envy of the eukaryotes.

LAMBDA PHAGE

How Gene *N* Works

from our Cell Biology Correspondent

It seems to be inevitable that as molecular biologists probe deeper and deeper into the biology of this or that phage or into the chemistry of some biosynthetic process their work becomes increasingly inaccessible to their colleagues not least because of the artificial barriers which arise as jargons evolve. Lambdologists, whose discoveries have laid bare in extraordinary and fascinating detail the complicated biology of bacteriophage lambda, may seem particularly prone to lapse into a shorthand language which rapidly becomes almost totally incomprehensible to the outsider. But more often than not what they have to say is of general significance.

The elegant experiments of Georgopoulos reported in the current issue of the *Proceedings of the US National Academy of Sciences* (68, 2977; 1971) are a case in point. When a phage infects *Escherichia coli* it faces the problem of expressing its genetic information in a regulated way using, initially at least, the host cell's RNA polymerase to transcribe early phage messenger RNAs. Phage-like T7 and T3 apparently specify soon after infection a completely new RNA polymerase which specifically transcribes the late phage genes, something which the host enzyme is apparently incapable of doing. Phage T4, on the other hand, seems to achieve the same end by modifying the RNA polymerase of the host cell and by making a new sigma factor to subvert the cell's enzyme to the transcription of the phage chromosome. How does lambda phage solve this problem? It has been known for many years that the *N* gene of λ provides a positive regulatory element, without which the phage cannot express most of its genome and complete its replication cycle. Clearly the *N* gene product might be a new RNA polymerase, or it might somehow modify the host polymerase, or it might act not on the polymerase but on some other part of the transcription machinery. The experiments which Georgopoulos reports strongly indicate that the *N* gene product acts in the second of these three possible ways.

Georgopoulos has isolated a suite of mutant bacteria which do not support the complete replication of wild type λ and has shown that at least some of these mutants are growth minus (*gro*⁻) because they prevent the *N* gene from functioning (*groN*⁻). For example,

λ *tryp* phage, which carry the *E. coli* tryptophan synthesis genes, can make tryptophan enzymes if the *N* gene of the phage can act normally. In other words, the amount of tryptophan enzymes made is a measure of the extent of *N* gene function. When *groN*⁻ cells are infected with λ *tryp* the phage fails to replicate and it also fails to specify the tryptophan enzymes. Apparently, the *groN*⁻ mutation in the host genome results in a modification of the target of the phage's *N* gene product such that this target molecule is no longer susceptible to attack by the *N* gene product. And by using *groN*⁻ cells Georgopoulos has succeeded in isolating mutant λ phage which manage to replicate in *groN*⁻ cells; these phage mutants fall into two classes, phage which, because of a deletion, have lost their requirement for *N* gene activity, and a second, more interesting class of phages which carry a mutation in their *N* gene which alters the *N* gene product and allows it to interact with the altered target in *groN*⁻ cells. The mutation in the *N* gene, in other words, compensates for the *groN*⁻ mutation in the cell's genome.

But what is the cellular target of the

N gene product which is changed by the *groN*⁻ mutation? By transduction experiments Georgopoulos showed that *groN*⁻ mutations are very closely linked to the locus which confers resistance to rifamycin. This locus is known to specify one of the subunit polypeptides of RNA polymerase; perhaps therefore the *groN*⁻ mutations are also in a locus which specifies part of the host's RNA polymerase. Georgopoulos therefore isolated RNA polymerase from wild type and *groN*⁻ *E. coli* and showed that *in vitro* the two enzymes do indeed differ in their response to changing ionic strength and in their sensitivity to inhibition by rifamycin.

There seems little doubt that the *groN*⁻ mutation changes *E. coli* RNA polymerase and it follows therefore that the host enzyme is in all probability the target of the *N* gene product. The question now, of course, is what does the *N* gene product do to RNA polymerase? Does it render the enzyme capable of initiating transcription at previously inaccessible sites on the genome or does it cause the enzyme to fail to respond to termination signals which in the absence of *N* gene product curtail transcription of most of the λ genome?

Heterogeneous Precursors to Ribosomal RNAs

THE genomes of plants and animals contain anything from about 200 to about 30,000 copies of the genes which specify ribosomal RNAs and as far as can be judged in each species these multiple copies are all identical. A structural gene for 18S ribosomal RNA (rRNA) and a structural gene for 28S rRNA are transcribed into a single long polycistronic RNA precursor molecule which, as is now known, is cut by a series of specific nucleolytic cleavages to yield the mature rRNAs and so-called spacer sequences which are apparently discarded. For reasons that are obscure, the amount of excess spacer RNA seems to be correlated with the organism's evolutionary position. There is more of it in warm blooded animals than in lower vertebrates, for example, and as Grierson and Loening suggest in next Wednesday's *Nature New Biology* (January 19), even within one organism the amount of spacer RNA transcribed may vary from tissue to tissue.

By gel electrophoresis Grierson and Loening analysed the maturation of ribosomal RNA in leaves and roots of the mung bean, *Phaseolus aureus*, and reach the conclusion that the largest pre-ribosomal RNA that can be detected in the leaf is about 600 nucleotides larger than that in the root. Although definitive proof that the primary product of transcription in leaf and root differs in length depends on finger print

analyses, and even then it will be hard to eliminate absolutely the possibility that in the root the transcript RNA is cleaved before its synthesis is completed, their data, together with the report of Tiollais *et al.* that precursor ribosomal RNA in mammalian cells is heterogeneous (*Proc. US Nat. Acad. Sci.*, 68, 1117; 1971), strongly suggest that identical RNA genes in different tissues of the same organism may be differently transcribed. If this is the case there must presumably be two or more initiation or termination sites for RNA polymerase in each ribosomal RNA transcriptional unit.

Also in next Wednesday's *Nature New Biology* van der Helm and Krakow report experiments which suggest how streptolydigin inhibits RNA polymerase of *Escherichia coli*. This antibiotic blocks both the initiation of new RNA chains and the elongation of chains already initiated. Van der Helm and Krakow believe their experiments show that it freezes or stabilizes the complex of RNA polymerase and its template so as to distort the enzyme. As a result, they suggest, the orientation of the 3' hydroxyl group at the growing end of the RNA chain is altered such that it can no longer react to form a diester bond with the phosphate residues at the 5' position of the incoming nucleoside triphosphate and so transcription ceases, as does the translocation of the enzyme along its template.

The Impact of Computer Graphics

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This article outlines some of the problems associated with the use of computer graphics and describes some applications in the physical and biological sciences.

WHEN computers were first introduced they were intended to tabulate solutions of equations that could not be handled by classical mathematical methods. The story of their subsequent application to non-numeric and commercial problems is well known, but it is less well known that even in the solution of numerical equations, much of their value was unexpected. For example, while using the first EDSAC computer at Cambridge to solve a radio wave scattering problem that had been considered intractable for many years, I was surprised to notice that one particular output value remained invariant to changes in the model. After some experimentation, this value was identified empirically, and led eventually to a sophisticated reformulation. The computer received no acknowledgment, for I was ashamed to admit that I had apparently wasted many nights of valuable computer time on a problem subsequently found to have an analytic solution; nowadays, finding algebraic solutions is sometimes quoted as an extreme example of the lengths to which some programmers will go to avoid an assignment!

This concept of numerical experimentation can be established as a continuum of the traditional theoretical and laboratory approach to science. I was once called on to check a complex statistics program in the shape of two suitcases full of Fortran cards. A few days work sufficed to develop a neat and quick test program which could generate simulated experimental data from a trivial model with an obvious interpretation and, sure enough, the statistical analysis failed, providing symptoms which allowed the fault to be located without too much difficulty. Thus the thinking scientist can be aided by a computer in devious ways, and the power of the partnership can be enlarged by improved man-machine communication facilities.

Hard Copy

Computer output can be classified conveniently into four groups. They are hard copy alphanumerics (for example, lineprinter, teletype and microfilm), hard copy graphics (for example, plotters and numerically controlled machine tools), soft copy alphanumerics (cathode ray tube displays) and soft copy graphics.

Even hard copy alphanumeric devices can be pressed into service; crude graphics, for example, can be drawn up by a lineprinter if a resolution of just under one per cent is adequate. They are particularly useful if a grey scale (half-tone) output is desired. Blank is used for white, dot for a pale grey, then slash, 'x' and finally 'm' or 'w' for the darkest shade. With computer microfilm output the effect can be enhanced enormously by defocusing the prints, so that each character blends into its neighbour. Mechanical

prints tend to be less effective because the impact brings high pressure to bear on small characters, thus yielding a darker shade of grey. Many printers, however, offer a no-line-feed option, so that characters can be overprinted, for example, a combined 'm' and 'w' for black tones.

For simple black-and-white output, a much finer resolution is offered by a modern device which has a row of six hundred or so computer controlled styli used in an electrostatic writing process. Graphics or drawings for output must be stored, then fed row by row as the paper advances. The more usual plotters produce line drawings in a calligraphic manner, the pen tracing out the drawing line by line as it is formed. The paper is mounted on a drum, which can be advanced or rewound in incremental steps of a few thousandths of an inch and the pen can be moved in similar steps to and fro across the drum. The effect is shown, on an exaggerated scale, in Fig. 1.

The applications of this simple computer controlled pencil and paper are legion. Much effort has been devoted to the development of special software, making plotters readily available to users with little time for the intricacies of computer hardware; this software includes a classic algorithm enabling a plotter to draw a best fit to a straight line in any specified direction—it involves only one add operation and tests at each stage¹. With two further "add" operations it is possible to generate ellipses or other conic sections or spirals as in Fig. 1.

Hard copy three dimensional output is less widely used, perhaps because of the problems of storage space. An

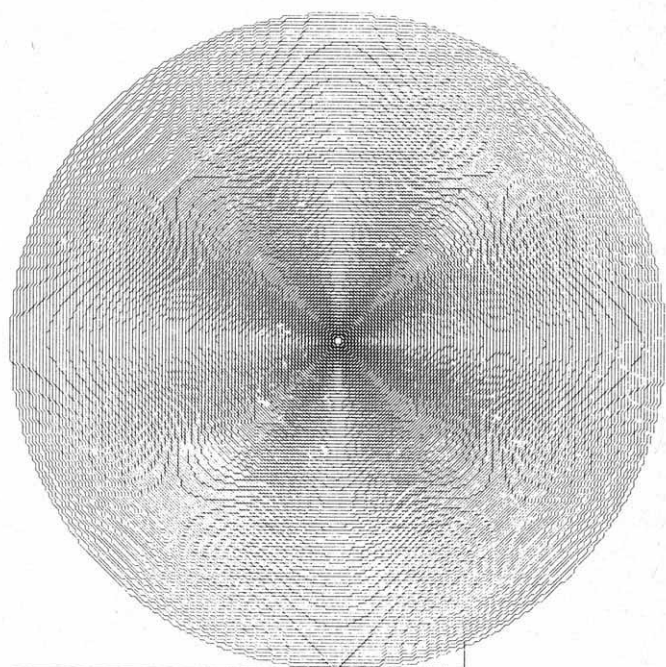


Fig. 1 Output from an incremental plotter.

extraordinary device has been developed using wire and a block of polystyrene; for example, at a computer command, a head runs to a new point on the block, pushes wire through it as far as required, cuts and moves on, thus gradually forming a surface in the fashion of a fakir's bed. Numerically controlled machine tools are possibly more promising, though it might be advisable to use polystyrene or some such soft material in case of program error.

Simple perspective drawings are adequate in many instances (in stereo pairs if necessary). It is a simple enough matter to have a computer draw a "wire-frame" three dimensional object in true perspective, but deciding which lines would actually be visible and which hidden (for an opaque object) proved surprisingly difficult. Early algorithms were prohibitively slow, the computer time taken varying as N^2 , where N is roughly the number of objects to be processed; some recent algorithms offer $N \log N$, allowing more complicated pictures to be processed. Exactly similar improvements have been achieved in the calculation of Fourier transforms (the FFT) and in the sorting of trees; it would be interesting to postulate similar improvements elsewhere.

The perspective drawing of mathematical surfaces, as in Fig. 2, can be programmed as a simple special case. The surfaces may be constructed one line at a time, and hidden lines are determined simply by comparison with an array recording the gradual covering of the paper. If, as the picture is drawn, a new point advances further across the page, the lines to it are drawn and the array updated; otherwise the pen is lifted and moved without drawing.

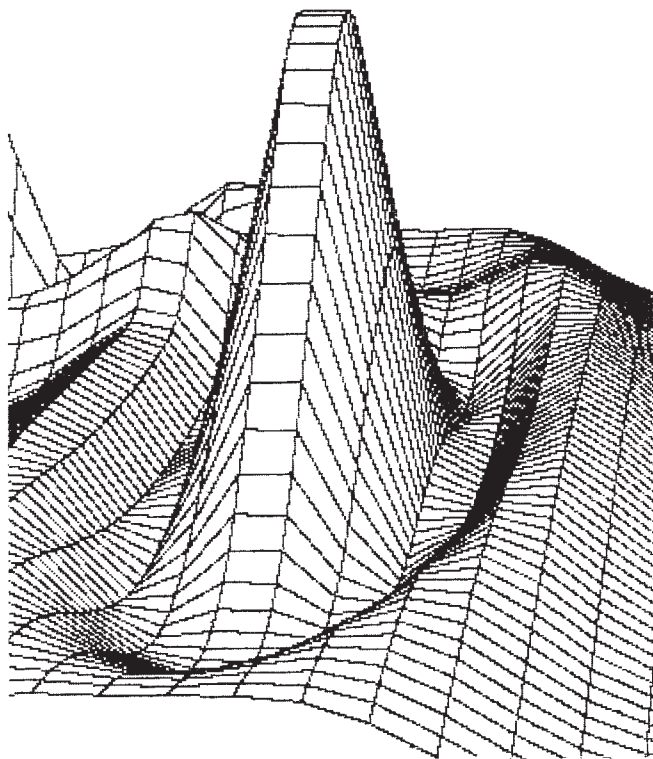


Fig. 2 A computer drawn surface.

Animation

The production of 30 s of ciné film involves the drawing of 700 individual pictures either directly onto the film or using an animation table. The technique is time consuming and expensive, but is valuable in numerical experiments for research and teaching. With the current proliferation of published scientific material a research worker must be prepared to sell his wares actively, and a ciné film can be a real asset for demonstrations and lectures.

By this technique one can see magnetic field lines linking and breaking in a conducting vortex, watch electrons hitting a potential well, watch a heart pumping blood, and drive a rocket car down a road between lamp-posts at relativistic speed (if you approach a red traffic light fast enough it will change to green by Doppler shift). I have used the technique to see how phase patterns move, linking and breaking in a complicated manner not unlike the behaviour of contour lines as height is changed through a saddle point, and have used the grey scale technique described previously to simulate a complex radio wave ground pattern experiment. In molecular biology, the technique has been used to simulate the unwinding of a helical polymer (DNA).

Soft Copy

Graphic display devices, usually employing some form of cathode ray tube, can be used for man-machine communication in perhaps its most sophisticated form so far. But costs still tend to be high.

In its simplest form, a picture drawn under computer control on a cathode ray tube appears only for a fraction of a second, and must be redrawn up to fifty times a second to maintain a continuous image. Thus movement can be possible in "real time" (it may take days to generate the frames for a short movie) though the computer may be too busy redrawing the picture to have time for any other calculation.

As with the hard copy devices, displays can operate in either a raster or a calligraphic manner. In raster mode, the

picture must be formed and stored to be written row by row in the manner of an ordinary television tube; only simple mass produced hardware is necessary, but programming is relatively difficult (though partially compensated by the possibility of grey scale). In a calligraphic system, the electron beam is driven across the tube to paint the successive lines forming a picture in any order at the convenience of the programmer.

Most displays are designed to achieve a resolution of 9 to 12 bits (1 part in 500 to 1 part in 4,000 in both X and Y directions) though finer resolutions are possible with laser beams. If we take a 10 bit resolution for discussion, rather more than a million individual points may be selected for display. Refreshing a black-and-white picture 50 times a second in a raster scan implies a data rate of 50 MHz, an impressive load for any machine. Similarly if a line drawing has to be picked out one spot at a time, a typical drawing requires the selection of some 10,000 spots, 50 times a second—again an enormous repetitive workload. Such peripherals may be relatively cheap to install, but will be expensive in computer processing time when in operation.

The problem may be tackled in two ways: either (1) some form of intermediate storage must be introduced to save the main computer from the refreshing process, or (2) more sophisticated hardware must be used so that a basic machine instruction can achieve more, as for instance with an alphanumeric display; the computer specifies the character, which is generated by special hardware.

A storage tube offers one possibility for intermediate storage but, unfortunately, once a picture has been generated no part can be erased without redrawing the whole, so we have in effect little more than a fast plotter. Recently progress has been made with a special storage wafer, viewed through a raster scan tube, which overcomes this shortcoming; the whole system costs little more than a regular plotter, and shows considerable promise. A more expensive alternative is to buy a small computer to be dedicated to refreshing the display; this offers powerful interactive facilities.

As far as the second possibility is concerned, one could use a very simple calligraphic display which accepts, one at a time, computer commands specifying spots to be displayed on the screen, say 10 bits for X and 10 for Y . This might just suit some applications, for example the mathematical model "life"². Most displays, however, now offer some form of line generating hardware, so that a computer can specify a straight line in some direction, for example through the coordinates of the endpoints (40 bits in all), or, more usually, through increments δx , δy , assuming that the line is to be drawn from the previous endpoint; sometimes δx and δy are limited to ± 63 or even ± 15 increments. Special commands may be used to turn the beam on or off to start a new sequence, or on some displays to vary the intensity of the lines.

The lines themselves are generated by special hardware, and in this context it is interesting to note that both analogue and digital techniques are commonly used. In the digital hardware, the beam is steered from point to point in a manner obviously analogous to the incremental plotter moves shown in Fig. 1. This is most effectively achieved by a special hardware implementation of the Bresenham algorithm¹, involving one additional operation for each move; because the electron beam is normally only moved about one increment each microsecond there is plenty of time available for this simple digital arithmetic.

Curves can, of course, be represented as a sequence of straight line increments; an ellipse, for example, can be generated by a program involving a simple matrix operation to compute each successive vector which will be output for the display (only the naive programmer need involve the computation of sines and cosines in this recurrence relation). The digital or analogue hardware can, however, be extended to permit the specification of a conic arc through an appropriate single command, just as the Bresenham algorithm itself can be extended.

An experimental display developed at Brunel University offers some of these options; it also includes another very simple arrangement allowing more general curves to be drawn quickly and efficiently with relatively few data bits, but this is beyond the scope of the present article.

For work in three dimensions, stereo viewing is usually resorted to, although the possible uses of holography or projection onto a spinning disk have been suggested. An optical system is normally used to separate the images and to enhance the effect. Two systems are particularly worthy of note because they allow several workers to gather around a display together, instead of taking turns to peer from some set point. One device, the lorgnette, consists simply of a rotating mask placing polarizing filters alternately in front of the viewer in synchronization with the display refresh cycle; the program is written to refresh alternate stereo images on alternate filters—a technique which can, incidentally, be used equally well to achieve colour effects. In the other system, developed by Professor Elliot and his colleagues at Imperial College, a glass plate is mounted horizontally in front of the screen. One image, displayed on the lower part of the screen, is viewed through the glass, which is backed with a polaroid sheet; its stereo partner is displayed inverted above, and its reflexion viewed in the glass, the natural Brewster effect being enhanced by another sheet of polaroid. The users wear polaroid glasses and view from any direction.

Interaction

To complete the man-machine graphics cycle, it is necessary to provide some graphic input device; it would be extremely laborious to code the coast line of a map, for example, as a series of coordinates or incremental directions.

A simple form of input allows the user to move a pen across a sensing tablet, or perhaps some form of pointer on

wires, equipped with hardware capable of reading out X , Y coordinates in digital form. A computer could be programmed in a loop continuously reading out X , Y coordinates to follow every movement of a man's hand, or could be equipped with an interrupt button so that the user may specify the ends of lines or other special data points which he wishes to input. With a little ingenuity, the output role of an incremental plotter can be reversed if the pen is replaced by a photocell which can be sensed by a computer; under program control, the "pen" can be driven across the drum searching for and tracking lines on a drawing for input.

A typical application of such a system is for modelling the ionosphere. Radio physicists often wish to use a computer to study the properties of an ionosphere specified by an electron density "profile", showing how the number of free electrons which effect radio waves in the upper atmosphere vary as a function of height above the Earth. Describing such profiles through tables of numbers is extremely tedious and prone to error, whereas profiles can be shaved and modified easily with almost any form of graphic input device.

A quite distinct form of graphic input can be made available with a calligraphic refreshed display. A "light pen" containing a very sensitive photocell is connected back to the computer system through an interrupt line. The user may point at some object on the screen, and the computer is thus interrupted during the refresh cycle at the appropriate moment. This is very convenient in certain applications because the user can identify some choice on the display screen without resorting to a program search through the complete picture to find the appropriate X , Y values. Graphic input with a light pen is less convenient because the computer must display some pattern to track the movement of the pen across the screen. Similarly, it is less convenient to use an X , Y coordinate device to point at some displayed object, although with a refreshed display special hardware can be built to match the input coordinates with the X , Y values driving the display.

The most powerful systems involve a display processor, effectively a special computer dedicated to refreshing and user interaction. Such systems permit pictures to be described through complex data structures, so that sub-pictures can be described and used in several places, possibly with variation of scale, orientation and other parameters. Only those parts of the picture actually visible need be projected onto the screen, which thus may be used as a "window" to zoom into the detail of a complicated picture which might involve many square miles of paper drawn out in hard copy.

With a system of this type the study of three dimensional objects can be reconsidered. The stereo projection techniques described previously can be combined with an appropriate input device, thus permitting the image of, for example, a complicated molecule to be turned and viewed from any angle. The translation and rotation operations involved can be conveniently described through matrix operations in homogeneous coordinates (scale is included as a fourth coordinate in place of ordinary Cartesians), which can again be assisted by special hardware. In its most sophisticated form so far, the user actually wears a display mounted to follow every movement of his head, so that he may wander at leisure around or through illusionary objects³. As with the less sophisticated facilities described earlier, interactive graphics is of such general application that it is meaningless to attempt any kind of exhaustive list but a few possibilities are detailed in a recent book⁴.

¹ Bresenham, J. E., *IBM Systems Journal*, 4, 25 (1965).

² Gardner, M., *Scientific American*, 224, No. 2, 112 (1971).

³ Sutherland, I. E., *Scientific American*, 222, No. 6, 56 (1970).

⁴ Paskow, R. D., and Green, R. E., *Advanced Computer Graphics* (Plenum Press, London and New York, 1971).

When does Information become Knowledge?

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By considering a specific example from molecular biology, Dr Wyatt examines the factors that determine the transformation of scientific information into scientific knowledge.

It is generally accepted that molecular biology began with the paper by Avery, MacLeod and McCarty¹ in 1944. The textbooks often link that paper with the equally well known although perhaps seldom read paper published in 1952 by Hershey and Chase². The next year the double helix structure for DNA was proposed by Watson and Crick³ and molecular biology became established. The history of these first ten years has been viewed through eyes perhaps mellowed by hindsight and wishful thinking but has not been examined in the light of the published work (as opposed to the memoirs) of the time. The fact that Avery was not awarded a Nobel Prize seems strange unless his work was less well known or was less recognized than we might now suppose. In this article I shall examine the information content of Avery's paper, the citations to it and the recognition it received in reviews and books.

Information Sources

Nowadays we may become acquainted with a paper directly or through a number of secondary sources such as *Science Citation Index*, *Current Contents*, keywords in *Biological Abstracts*, through subject groupings in *Biological Abstracts*, *Index Medicus* and more specialized sources like *Genetics Abstracts* (see ref. 4). Although there were fewer aids in 1944, the keys were similar and there were fewer papers to find.

The paper by Avery *et al.* was published in *Journal of Experimental Medicine* from the Rockefeller Institute where Avery worked. It was a famous and respected journal bought usually by medical rather than science libraries, but at that time most geneticists worked in science departments. In 1944, there were thirty-six copies of the journal in libraries in Britain and of these at least twenty and probably more than twenty-six were in medical or veterinary libraries⁵. In universities like Leeds the medical school and its library are separated from the main campus. In London the only copies apart from those in medical libraries were in the libraries of the Chemical Society and the University of London, which was a closed access library. The Second World War would also have exaggerated the usual delays in delivery of the journal outside the United States.

In libraries in the United States itself there were about 125 copies⁶ of the *Journal of Experimental Medicine*. Of these at least forty-five were in medical libraries, probably seven in the libraries of pharmaceutical firms and seventy in university libraries. Some of those in university libraries were probably housed in separate medical buildings; there were at least two in agricultural libraries. The records of the journal show that there were about 600 paid and 100 complimentary copies in the United States. Thus in the United States, this journal may have been more widely accessible to geneticists than in other countries.

Title and Conclusion

The title (Table 1) contained few if any key words that would have led to recognition at the time. Transformation was the

term used in the various papers on the subject, all published in medical science journals since Griffith's paper⁷ in 1928. "Transformation" also described the work of Berry and Dedrick⁸ with Shope's virus. These experiments were known to geneticists but were regarded more as curiosities than part of the mainstream of genetics. Similarly, geneticists did not believe that deoxyribonucleic acid was of great importance in heredity. A reader, therefore, who had glanced at the title might well have missed the significance of the paper, and the conclusion (Table 1) would not have enlightened him.

Table 1 The Title, Summary and Conclusion of Avery *et al.*¹

Avery, O. T., MacLeod, C. M., and McCarty, M. Studies on the Chemical Nature of the Substance Inducing Transformation of Pneumococcal Types. Induction of Transformation by a Deoxyribonucleic Acid Fraction Isolated from *Pneumococcus* Type III.

SUMMARY

1. From Type III pneumococci a biologically active fraction has been isolated in highly purified form which in exceedingly minute amounts is capable under appropriate cultural conditions of inducing the transformation of unencapsulated R variants of *Pneumococcus* Type II into fully encapsulated cells of the same specific type as that of the heat-killed microorganisms from which the inducing material was recovered.

2. Methods for the isolation and purification of the active transforming material are described.

3. The data obtained by chemical, enzymatic, and serological analyses together with the results of preliminary studies by electrophoresis, ultracentrifugation, and ultraviolet spectroscopy indicate that, within the limits of the methods, the active fraction contains no demonstrable protein, unbound lipid, or serologically reactive polysaccharide and consists principally, if not solely, of a highly polymerized, viscous form of deoxyribonucleic acid.

4. Evidence is presented that the chemically induced alterations in cellular structure and function are predictable, type-specific, and transmissible in series. The various hypotheses that have been advanced concerning the nature of these changes are reviewed.

CONCLUSION

The evidence presented supports the belief that a nucleic acid of the deoxyribose type is the fundamental unit of the transforming principle of *Pneumococcus* Type III.

Summary and Discussion

The summary shown in Table 1 was, again, not very provocative—there was no mention of gene, mutation or any terms to link the findings to general genetical ideas. The discussion was more than three pages long and the last half page dealt with the genetic interpretation. Three current views of the nature of the transforming substance were put forward; namely, that it had been likened to a gene, that it was like a virus and that it was a "transmissible mutagen".

Abstracting Journals

Although the significance of the work was not immediately evident from the title and from the paper itself, it could have been made clearer in the abstracts published in *Biological Abstracts* and *Chemical Abstracts* in 1944. In the first the abstract prepared by McCarty was in the "Immunology, General and Bacterial" section. It did not make clear any genetic importance, nor did the abstract in the "Microbiology" section of *Chemical Abstracts*. In the subject index it could

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have been found through "desoxynucleic acid" and "Pneumococcus" in *Chemical Abstracts* and under "nucleic acid" and "Diplococcus" in *Biological Abstracts*. Anyone searching under genetical terms such as acquired characters, crossing over, dissociation, genes, genetics, genotype, heredity, hybrid, mutation, phenotype and variation, would not have tracked down the paper through either abstracting journal. For retrieval purposes, the paper was evidently considered only from the narrower microbiological point of view. In fairness to the abstracting journals, however, the authors did not provide the leads in their paper and McCarty wrote an abstract which ignored the genetic aspects. It would have been a bold subject editor who would have filed it under "mutation" or "hybrid". It is in such a case that the weakness of the abstracting and indexing journals is most clearly seen. Thus an eager searcher in the secondary journals would have had difficulty in finding the paper and the abstract itself would not have been very revealing.

Professor Coburn has written recently⁹ of his friendship with Avery and states that Avery was well aware of the implications of his discovery. This is not so evident from Coburn's narrative. It was eight weeks after his conversation with Avery that Coburn himself realized the implications, which he did not enumerate in his letter to Avery. In a long letter written on May 17, 1943, to his brother, Avery clearly says that his transforming principle (DNA) "sounds like a virus but may be a gene". This letter is reproduced in full by Dunn¹⁰ and by Hotchkiss¹¹. Sir MacFarlane Burnet, on a visit to the United States from Australia, spoke with Avery at the Rockefeller Institute a month after the paper was submitted for publication. Burnet wrote to his wife in December 1943, "Avery . . . has just made an extremely exciting discovery, which, put rather crudely, is nothing less than the isolation of a pure gene in the form of desoxyribonucleic acid"¹².

We may assume then that the lack of a clear statement of the importance of his discovery in his paper was due to Avery's "constant modesty and deep humility . . . (whose) high regard for the printed word deterred him from theorizing in print"⁹. His modesty also ensured that the citations contained none which would have firmly linked it to genetics, a reference, for example, to Beadle and Tatum's paper¹³ of 1941. Whatever Avery thought of his work, he intended it to be found, seen and read by those interested in pneumococci, not genetics.

Nevertheless the news spread fast. Burnet records that he next visited Cold Spring Harbor and then Beadle and Tatum at CalTech. Demerec, the director of the Cold Spring Harbor Laboratory, was a member of the phage group and another member, Luria, has said that he was well aware of Avery's work before publication¹⁴. Neither the geneticists nor the phage group, however, appeared to be greatly influenced by the news. But Erwin Chargaff of nearby Columbia University in New York was moved to take up the study of nucleic acids^{15,16} and his later work provided an important clue for Watson and Crick.

In an address to the Royal Society in November 1945 on "The Gene", Muller¹⁷ devoted about 3 per cent of his time to "possible roles of the nucleic acid". This brief review, summarizing Avery's work, listed a number of interpretations but left open the question of whether the specificity resided in the nucleic acid polymer or in an associated protein.

Cold Spring Harbor

The 1946 symposium, the first since 1942, was on heredity and variation in microorganisms. There were twenty-seven papers including one by McCarty, Taylor and Avery¹⁸. Of the 136 participants from six countries, eleven came from the Rockefeller Institute. Six of those present later became Nobel Laureates. One of the four attending from Britain was Pirie from Rothamsted Agricultural Research Station which had the *Journal of Experimental Medicine* in its library. Four of the speakers, Dienes, Hershey, Kidd and Luria, made six references to the three papers by Avery and his co-workers in the *Journal*

of *Experimental Medicine*^{1,19,20} and Anderson referred to the presentation at the symposium. Spiegelman made three general remarks about Avery's work without giving citations.

It is therefore surprising that the next symposium, in 1947, on nucleic acids and nucleoproteins was not attended by Avery or MacLeod or McCarty. Chargaff mentioned that "the epochal experiments by Avery and his associates have emphasized the very important role by some bacterial nucleic acids in the determination of inherited synthesizing ability"¹⁵. The next relevant symposium was in 1951, on genes and mutations. Nucleic acids and DNA were not mentioned except in discussions, until pages 445 to 460 when there were two papers on transformation. Here it was clear that the authors were thinking of the DNA as synonymous with the gene.

Even at the 1953 symposium when Watson and Crick presented a paper on DNA there was little mention of Avery and his associates. There were four groups of biologists who were or should have been interested in Avery's findings—the biochemists, the geneticists, the microbiologists and the phage group. Their various paths came together with the Watson-Crick structure for DNA, as told by Asimov²¹.

Biochemists

Chargaff, inspired by Avery's work, made many significant researches into the structures of nucleic acids. The most important was the overthrow of the tetranucleotide theory of Levene. Based on the equimolecular proportions of the four bases, this model proposed that the DNA structure was a sugar-phosphate backbone with alternating purines and pyrimidines in groups of four. Such a structure could have little specificity, especially compared with the proteins known to be associated with chromatin. Chargaff and his associates showed that the DNA is characteristic for each species and that the ratios of A : T and C : G were nearly equal but that the ratio of A : T : C : G varied. This made DNA a much more interesting molecule¹⁵ and provided a vital clue for Watson²².

Geneticists

It is not surprising that the geneticists were little interested in DNA until Chargaff had shown the tetranucleotide theory to be misleading. The evidence came from one heritable change in one organism and that organism was a bacterium, a class widely believed to have very different genetic properties from other classes of organisms. It was several years before other examples of transformation were shown in *Pneumococcus*, *E. coli* and *Haemophilus* (see ref. 11). For the classical geneticists, transformation was not a system which could be used in their experiments: it was therefore difficult for them to integrate transformation into their ideas. Beadle records²³ that the one-gene-one-enzyme hypothesis was still largely unaccepted in 1951. Thus a key experiment was described as a curiosity in the textbooks (see below) and ignored by the scientists in general but seems to have been kept to the fore largely in semi-popular lectures.

In a mammoth ninety page summary of the symposium on phosphorus metabolism held in 1951, however, Bentley Glass devoted three pages to DNA in bacterial transformation²⁴. He wrote: "Transforming agent, in all likelihood DNA, is akin to if not identical with the genetic units. This is the strongest support for the view advanced by Zamenhof and discussed earlier, the view that the genes of organisms are to be found not in the proteins but in the myriad forms of the DNA itself."

About 150 people attended the meetings: Glass, a geneticist, only Herriot from the phage group, Hotchkiss, a microbiologist (who gave a very good account of transformation), Chargaff and Zamenhof who spoke on DNA; the rest were chiefly biochemists.

Phage Group

The phage group, as Luria records, knew of the work before publication but apparently did not act upon it even though one

explanation was that transformation was accomplished by a phage, not DNA. One reason was that Luria had proposed a theory which demanded a phage genetic material containing little DNA¹¹. In 1951, however, both Herriott (who had attended the meeting on phosphorus metabolism) and Northrop were suggesting that the DNA of phage might be like the transforming principle. Watson, who had been Luria's student, and Maaløe²⁵ had noticed that much of the labelled parental phage protein remained attached to cellular debris. The next year Hershey and Chase², as a result of their famous experiment, plainly suggested that protein had no function in phage multiplication and that DNA had. Their experiments showed that about 20% of the protein and about 75% of the DNA could not be removed from the infected bacteria, 80% of which were viable. Other experiments showed that less than 1% of the protein from parental phage was transmitted to progeny phage. Detection of proteins was limited to those containing sulphur (labelled with ³⁵S). Thus, as the authors pointed out, "whether sulphur-free material other than DNA enters the cell has not been determined". The authors' interpretation of the experiments, as given in detail in the results, discussion and summary, have mostly been vindicated. We now know, however, that essential proteins are injected with the DNA. Moreover a non-conservative replicating mechanism would mean that there would be no transmission of the radioisotope from the parent to the progeny. What is surprising is that the problem of contamination, which had been cited so often to criticize Avery's experiments, was not raised in the context of these phage experiments.

Summaries Compared

It is particularly interesting to compare the summaries of these two papers: Avery's has its non-committal résumé of the facts whereas Hershey and Chase's has its mixture of fact and interpretation—"8. The sulphur-containing protein of resting phage particles is confined to a protective coat that is responsible for the adsorption to bacteria, and functions as an instrument for the injection of the phage DNA into the cell. This protein has no function in the growth of intracellular phage. The DNA has some function." At the 1953 Cold Spring Harbor Symposium on viruses, Hershey discussed the limitations of the experiment.

Luria, who was to have been a speaker at a meeting of the Society for General Microbiology in the spring of 1952 at Oxford, England, was denied a passport by the State Department and Watson gave a talk instead. "Almost no one in the audience of over four hundred microbiologists seemed interested as I read long sections of Hershey's letter"²² describing the famous experiment. Unfortunately we do not know their interest in the work of Avery or Beadle and Tatum.

The Microbiologists

The recipients of the Eli Lilly award in bacteriology and immunology have included M. McCarty (chemical nature and biological specificity of the substance inducing transformation of pneumococcal types, 1946, ref. 26); S. S. Cohen (synthesis of nucleic acid by virus-infected bacteria, 1951, ref. 27) and J. O. Lampen (metabolism of nucleic acid components in bacteria, 1952, ref. 28). In spite of this interest in nucleic acids, other reviewers in *Bacteriological Reviews* were less than convinced until 1952 of the role of DNA in heredity. In a forty page review of recent advances in bacterial genetics²⁹, Luria devoted a page and a half to Avery and to later work on *E. coli* in a section on specific induction of mutations. He wrote "... prove biological specificity of nucleoproteins can be carried out not only in the protein but also in the nucleic acid moiety".

In a review of bacterial dissociation, W. Braun³⁰ devoted two lines to Avery and his co-workers. Cohen said that "evidence that DNA plays a genetic role ... while considerable and suggestive is at least indirect"²⁷; he did not cite Avery. A review of bacterial transformations by Austrian³¹ was,

however, more positive and he mentioned that protein contamination of the transforming DNA seemed to be ruled out by the experiments of Hotchkiss¹¹—an original objection voiced by Mirsky at the 1947 Cold Spring Harbor symposium. He continued by saying that "a growing body of increasingly precise evidence gives continuing support to the view that DNA is indeed the biochemical determinant of inheritable characters. Much indirect evidence supports the view that DNA is concerned with genetic mechanisms in all living forms".

Mirsky devoted a page to Avery's work in a report of a meeting held in 1950 (ref. 32), but nearly all of this was devoted to arguing that transformation was due to a protein impurity or a phage particle. He seems to have been unaware of Hotchkiss's work in the same institute.

Thus, by 1952, there were small numbers of influential people who had accepted the idea that DNA might carry genetic information. The 1951 Cold Spring Harbor symposium on genes and mutations contained only two papers, by Ephrussi-Taylor and Hotchkiss, specifically on transformation (DNA). But the discussions were more revealing. Lederberg asked "if it were possible that Hotchkiss was dealing with a suspension of chromosomes or fragments consisting primarily of DNA—this would permit crossing-over of the gene". Szybalski mentioned "Muller's suggestion that whole 'free chromosomes' or parts thereof are involved in transformation". Altenburg, however, asked if "it could be a contaminating virus with the DNA".

Textbooks of the period were less satisfactory. For example, Srb and Owen³³ wrote a good textbook in 1952. The index contained the terms *Pneumococcus* and transforming principle, but no mention was made of Avery. Less than one page was devoted to the pneumococcal experiments and transforming principle which "appears to be a nucleic acid" (page 248). But in the index, none of the words desoxyribose, DNA, nucleic acids, nucleoproteins (or RNA) contains an entry for that page. In the two pages on nucleoproteins it is mentioned that the nucleic acid of the "transforming principle" of pneumococcus is of the DNA class. The section on DNA mentions only nucleotides without detailing A, T, C and G.

Advances in Genetics, an annual which began in 1947, contains no reference to Avery's work until 1955 when it was mentioned twice in a review of microbial genetics by Bryson and Szybalski. Hershey wrote a review of inheritance in bacteriophage in 1953, mentioning his own experiments of 1952. Despite the use of the words desoxypentose nucleic acid and nucleic acid in the latter review and the Watson and Crick paper in 1953, the words DNA, desoxyribonucleic acid, RNA or desoxypentose acid do not appear in the indexes to the first eight volumes (including 1956) and the only entry under nucleic acids alludes to the effect of radiation on them. Evidently the indexers did not consider that these compounds were worth indexing even when their contributors used them. Avery's work was, however, quoted outside these areas. In the 1945 *Annual Review of Physiology*, Wright³⁴ devoted half a page of a 30 page review of the physiological aspects of genetics to Avery, with the rather ambiguous comment that "the great possible significance of this observation in the interpretation of the role of the nucleic acids of chromosomes and of other self duplicating entities is obvious".

Semi-popular Literature

In addition to the recognized scientific journals, there is a large amount of semi-popular literature which is peripheral to the mainstream. It is, however, difficult to judge its effect either in the long or the short term. *American Scientist* carried at least two articles which emphasized the importance of Avery's work. In 1945 Hutchinson³⁵ devoted a page to a discussion of Avery's recent paper: "It has been likened to gene or virus. An extremely fundamental contribution to all biological sciences." In 1948 Beadle cited Avery in a six page lecture on "genes and biological enigmas": "Pneumococcus

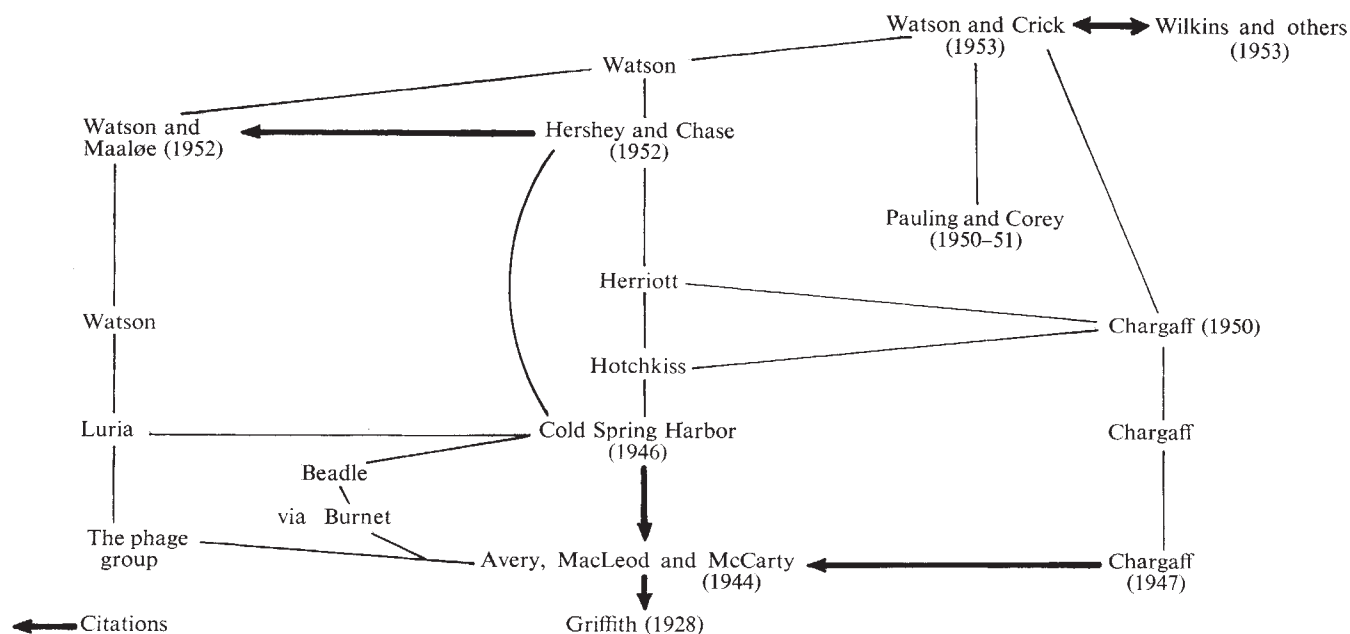


Fig. 1 Simplified picture of the influence, information and citation networks from Avery to Watson and Crick.

type transformations which appear to be guided in specific ways by highly polymerized nucleic acids may well represent the first success in transmitting genes in predetermined ways". Journals like this may influence more students and younger workers than do the primary journals or review series. Visiting lecturers to the campuses, like Beadle, may be more influential for change than the textbooks. The habitual constraints and form of the usual scientific paper become reflected in the textbooks and reviews. It seems that it is only outside the "normal" scientific serials and in discussions, whether public or private, that a freer and bolder view can be offered for debate.

Citation Networks

The invisible college of commuting scientists who visit and correspond with each other is well illustrated in this article and in Watson's book. The various influences are shown in Fig. 1. What is perhaps surprising is the lack of direct citation to the key papers. Thus, Watson and Crick³ did not cite Hershey and Chase² or Chargaff¹⁵ or Avery, MacLeod and McCarty¹. Similarly, Hershey and Chase² did not cite Avery's work¹. A little later Gierer and Schramm³⁷, who showed that the RNA is the infective (genetic) portion of tobacco mosaic virus (TMV), cited Hershey and Chase but not Avery and his associates. Garfield, Sher and Torpie³⁸ made a study of the citation data of the discovery of the genetic code using Asimov's book²¹ as the source. Although some of the citation links were good (for example, citations by Avery and his colleagues of the work of Griffith and other workers on transformation) the citation links elsewhere were few. Nor do recent scientists cite Avery: from 1966 to 1969 there was an annual average of seventeen citations to the 1944 paper and an average of four to the other three papers by Avery and his co-workers. Several such citations will have been in articles such as this one.

Information and Knowledge

There is no reason to suppose that with all our information aids today, a similar paper would be any better recognized than Avery's—indeed among so many more papers it might fare worse. Both Beadle²³ and Stent¹⁴ have suggested that new information can be assimilated only when it can be fitted without too much difficulty into accepted ideas. As information it is unrecognized until it is transformed into knowledge.

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Organic Microspheres from the Precambrian of South-West Africa

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Quartz crystals from the Precambrian of South-West Africa contain organic particles in their inclusions which closely resemble, in detailed morphology, coacervates, proteinoid microspheres, or fossil and recent microorganisms. The theoretical significance of these organic particles is discussed with reference to problems of the origin of life and the search for extraterrestrial organisms by remote sensors.

BITUMENS have been reported from later Precambrian formations of hitherto unknown radiogenic age of South-West Africa from 1919 onwards. According to the review of the literature by Martin¹ a gilsonite-like substance associated with calcite occurs in gushes of a brecciated sandstone of the Fish River Series of the Nama System in the Berseba Native Reserve. At Heigums No. 105 farm within the Bethanie district, a fault in the Kuibis quartzite was claimed to contain semi-solid pitasphalt. Studies by the author of the collection of the SW African Geological Survey at Windhoek revealed that the fault breccia from Heigums No. 105 farm contains both oil and solid bitumens. The bitumens within the occurrences are interstitial and are therefore likely to be contaminated.

Oil and bitumens hermetically preserved from contamination as inclusions under elevated pressure have been previously reported from Geiaus No. 6 farm, Klein Karas district. Martin gives the following geological description of the occurrence: "The quartz crystals" (with inclusions of water and oil) "are contained in calcite-filled gash veins in a pre-Nama diabase dyke (K. Schalk, personal communication) emplaced in metarocks of the Kheis System. Formerly this area was overlain, not far above the present level of exposure, by both the Nama beds and the Karoo beds, remnants of which are still preserved in the vicinity. The calcite-filled veins in the diabase are probably the result of post-Nama and/or post-Karoo faulting which has taken place along some of the north-south striking diabase dikes." I found during a field trip to the locality, which is an old test pit, that the principal 50 to 70 cm wide, 10 m long, vertical and approximately N to S striking vein is filled with massive calcite. The vein is traversed by 10 cm broad horizontal fissures in which there is a second generation of scalenohedral calcite with yellow fluorescence; radiating aggregates of pale blue fluorite with blue fluorescence, traces of pyrite and chalcopryrite, and rather flattened, colourless quartz crystals, up to 10 cm long. Only the last mentioned quartz crystals proved to contain organic inclusions within the 0.1 to 5 mm diameter range.

Material was also collected from another location within the Klein Karas district, which is not mentioned in the literature. This is a test pit with the exposure of a 60 cm wide and 5 m long calcitic vein in a diabase dyke in the Kheis System, which is situated some 11 km to the south-west of Althorn

Farm. Within this vein there are irregular vugs, lined with scalenohedra of calcite and up to 12 cm long, often doubly terminated crystals of quartz, which latter contain the bulk of the organic material as inclusions.

From mineral dealers at Windhoek there were purchased for the purposes of the present work quartz crystals with organic inclusions from the following geologically unspecified Precambrian localities: (A) up to 4 cm long, freely grown smoky quartz and citrine from the Klein Karas district; (B) up to 5 cm long, perfectly transparent crystals from the Berseba Native Reserve, which was mentioned¹ as a locality for interstitial bitumen; (C) a single, 25 cm long, freely grown crystal with up to 20 mm long inclusions from the Gruenau district.

It was observed² that the inclusions from all these localities contain organic particles with morphologies resembling in several details those of the primitive organism. These particles are described here.

Properties of Carbonaceous Complex

Previous chemical data exist regarding the entire included carbonaceous complex, which consists of both biomorphic and non-biomorphic organic matter, as shown below. According to an unpublished work by Wilson (1965) for the Artnell Exploration Co., quoted by Martin¹, "The oil in the quartz crystals seems to be derived from substances rich in chlorophyll". Kvenvolden and Roedder³ found through gas chromatography-mass spectroscopy analyses in quartz crystals from Geiaus and Aukam Farms, predominantly normal alkanes and about 5 p.p.m. of isoprenoids. The same workers concluded that the ¹³C/¹²C ratio of the inclusions is higher than that usually attributed to carbon from biological sources.

For the purpose of elemental analysis of the included carbonaceous complex, the ground quartz crystals were treated with 0.1 N HCl to eliminate traces of carbonate, and

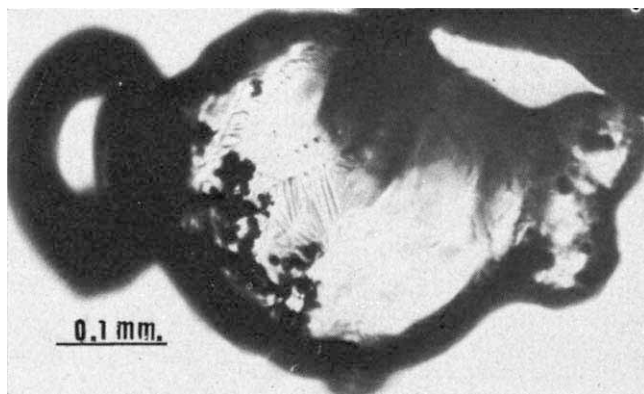
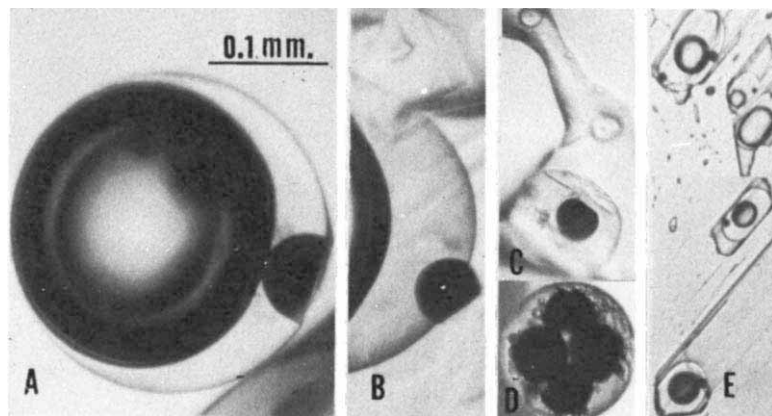


Fig. 1 Particle of vasinous oil covered with a crinkled skin and showing some buds. The particle freely floats in the watery phase of the large inclusion. It has an anchored gas bubble (left), a hemisphere of black bitumen behind the gas bubble, and a number of smaller microspheres of the brown bitumen. (In all illustrations the line is 10 μ m long, unless otherwise marked.) Geiaus Farm.

Fig. 2 Morphology of black bitumen. *A* and *B*, Hemispheres on surface of vaseline oil; the globules of brown bitumen flanking the black hemisphere of *A* are in oscillatory motion on illumination; *C* and *D*, spheres in oil with some smaller spheres of the brown bitumen; *E*, spheres in a number of inclusions the size of which seems to be proportional to the amount of oil present. Althorn Farm.



dried for 24 h at 105° C. Quartz from Geiaus Farm proved to contain 0.26% C and 0.07% H. Dr C. B. Moore of Arizona State University analysed a quartz sample from Althorn Farm (personal communication), with the following results: C=0.041%, 0.041%; N=0.0076%, 0.0089%; S=0.003%, 0.003%.

According to G. W. Hodgson (personal communication), the fluorescence and other indications for porphyrins are more intensive in extracts from the central portions of a quartz crystal from Althorn Farm than from an extract prepared from the close-to-surface zones of the same crystal. This seems to indicate that the porphyrins detected in the crystal cannot be surface contaminants.

For the purposes of study of the individual constituents of the carbonaceous complex, the quartz crystals were first surveyed under the binocular microscope. Subsequently plane-parallel slabs were cut from those portions which appeared to contain inclusions with structurally interesting particles. The polished slabs were examined and photographed under the Leitz petrographical, phase contrast, fluorescent microscopes and heatable-coolable stage. The inclusions are under pressure, and for this reason grinding too near them results in decrepitation, but by careful grinding and boring, a slow blow-out through a flaw can be effected, following which the inclusion can be opened and the spectra, solubilities and so on of the organic particles determined.

I found that each inclusion may contain water, gas and a suit of three types of organic substances, which are described below in order of their decreasing mean abundances.

(1) Vaseline oils are present as droplets and gas-filled blisters (Figs. 1–3). Their colour varies between pale yellow and pale green, the fluorescence varies between green and blue, the melting points in twenty inclusions ranged between

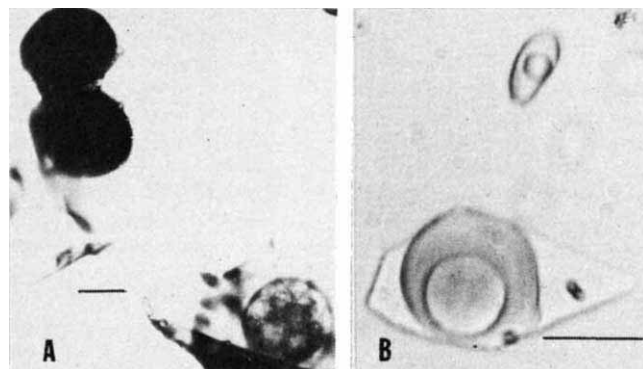


Fig. 4 Internal emulsions and vacuoles. *A*, Part of a large water filled inclusion with sphere of brown bitumen which contains a watery emulsion of droplets of the same bitumen beneath a relatively clear surface zone (bottom right) and two spheres of black bitumen sprinkled with microspheres of a light brown bitumen (top left); *B*, two inclusions containing brown bitumen globules with central vacuole of water. Althorn Farm.

–35° and +2°. The oil floats on the watery phase of the inclusions and it is readily soluble in CHCl_3 .

(2) Black bitumen forms opaque, non-fluorescent, hemispheres and spheres, which invariably show sharp boundaries against the other organic substances present within the inclusions (Figs. 1–3, 4 and 6). The proportion of black bitumen to oil seems to stay quasi-constant for all the inclusions in a given crystal (Fig. 2*E*). The density of the substance is greater than that of the watery phase of the inclusions. Most of the free particles partially melt between 350° and 450° C, and they are insoluble in CHCl_3 .

(3) Brown bitumens constitute the group of substances which form particles with the most striking microorganism-simulating morphologies (Figs. 1–10). Transitions between varieties of oil and varieties of the brown bitumens are more or less sharp (Figs. 1–3). The colour varies between buff and dark brown.

The predominantly organic composition of the biomorphological particles of brown bitumens is indicated with the following data. (A) Intensive fluorescence of yellow, orange, or brick-red colours, which tends to darken on heating to 300° C. (B) Charring and disappearance of fluorescence on heating of freed particles with an incandescent filament. (C) Most particles melt or soften between 80° and 150° C. (D) The particles float on the watery phase of the inclusions, and the free particles float on distilled water. This evidence seems to indicate that the particles may contain less than 10% inorganic matter, as all the conceivable mineral contaminants have densities in excess of 2.0. (E) The particles are insoluble in water and partially soluble in chloroform. (F) On heating of the inclusions, the gas bubble was found to disappear between 120° and 160° C as also observed by Kvenvolden and Roedder³. Before decrepitation between about 250° and 350° C, brown streaks are seen oozing into the oil

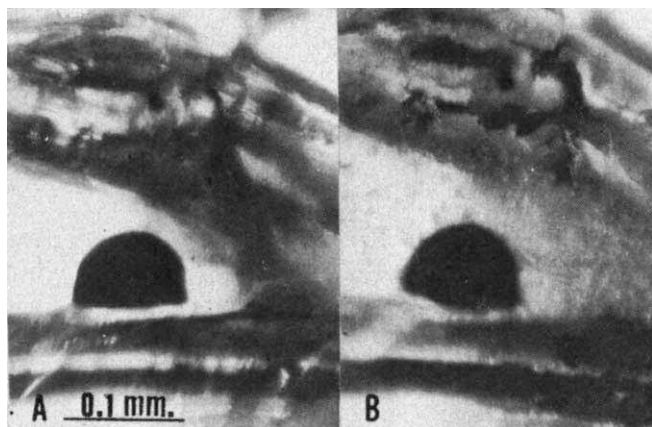
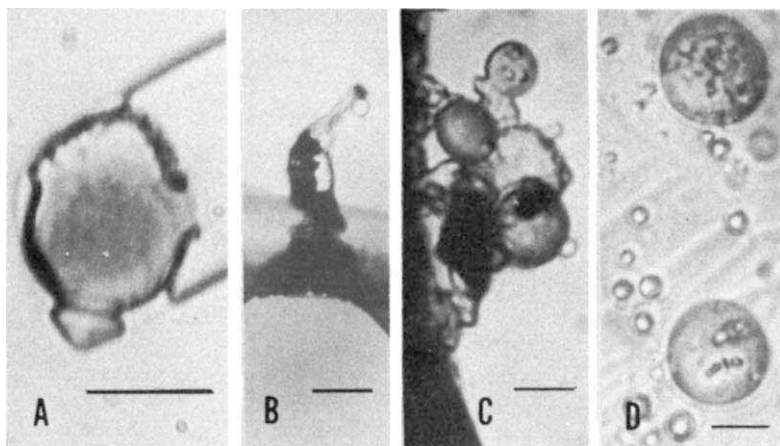


Fig. 3 Oil filled inclusion with hemisphere of black bitumen resting on the wall. *A*, At 20° C; *B*, at +5° C. Note the appearance of rough microspheres of a substance, which has optical properties similar to that of the brown bitumen. Geiaus Farm.

Fig. 5 Budding features. *A*, Incipient bud protruding from dark brown sheet covering the light brown microsphere; *B*, crinkled tubular structure which contains black and buff coloured bitumen; two spheres of the latter are protruding from the orifice; *C*, intertwined group of light brown and dark brown microspheres; the colour of the buds, with one exception, is identical to the host; *D*, part of water-filled inclusion with approximately 40,000 microspheres; budding is restricted to microspheres with diameters exceeding 6 microns. Althorn Farm.



from both the black and the brown bitumen, indicating their partial dissolution at elevated temperatures in the organic oily matrix. (*G*) Some oils of relatively dark yellow coloration from the Geiaus Farm locality precipitate rough microsphere like particles (with optical properties within the range of the brown bitumen) when cooled to $+5^{\circ}\text{C}$, which redissolve at 20°C (Fig. 3). It seems unlikely that any conceivable inorganic substance would have such a degree of temperature dependent solubility in the oil.

Preliminary work with the microspectrophotometer of Stroher and Wolken⁴ was carried out on both opened and intact inclusions, and the resulting visible and near ultraviolet

spectra have been interpreted by Professor J. J. Wolken (see Fig. 12). According to general experience in petroleum geology, the vasinous oil is anticipated to be a relatively pure mixture of saturated hydrocarbons, which would have no absorption bands within the spectral range in question. Twenty-three spectra of vasinous oils proved to show only a gradually increasing absorption towards the red, without any maxima. The spectra included those of droplets, illustrated in Figs. 2*A, B, C, E* and Fig. 3. The light brown, yellow to orange fluorescent particles of the brown bitumen showed a more or less pronounced maximum adsorption between 3500 Å and 4000 Å, which is indicative of lipids. The sixteen microsphere-like assemblages of brown bitumens which gave this positive spectral feature of lipids included microspheres illustrated in Figs. 4*A, B*, and 5*A, B, C*. The lipid absorption band was the most marked within microsphere assemblages which showed a tendency for chain formation, such as those illustrated in Fig. 8*A*. Finally, some of the dark brown, red fluorescent microspheres of the brown bitumen (Fig. 2*D* and Figs. 9*A, B*) showed indications of absorption within the vicinity of 4,500 μm which may be due to the conjugated double bond of possibly isoprenoid type substances.

Morphology of Particles of Brown Bitumen

The particles of the brown bitumen resemble proteinoid microspheres or microorganisms in respect of one or more of the following features.

(1) Structure: some of the individual microspheres may have sheaths (Figs. 1, 5*A, B*), vacuoles (Fig. 4), buds (Figs. 5, 6) and cores (Fig. 6). In some inclusions marked uniformity of the diameters of the microspheres may occur (Fig. 7*B*). (2) Types of aggregation: the microspheres may be grouped into couples (Figs. 7*B, C* and *D*), irregular aggregates (Fig. 8*A*), chains (Fig. 8) and gigantic lattices (Figs. 9, 10). (3) Rare structures: these include spirals (Fig. 11*A*) and regular combinations of

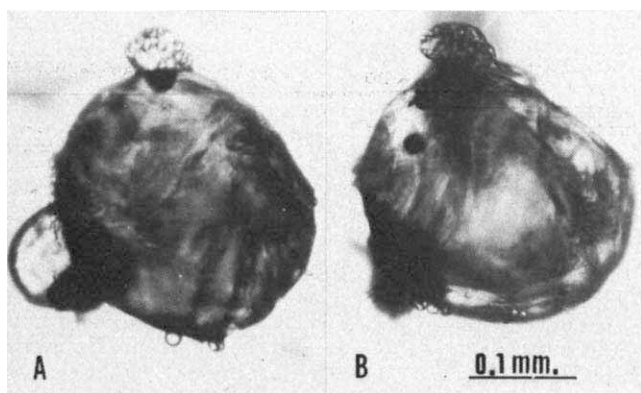
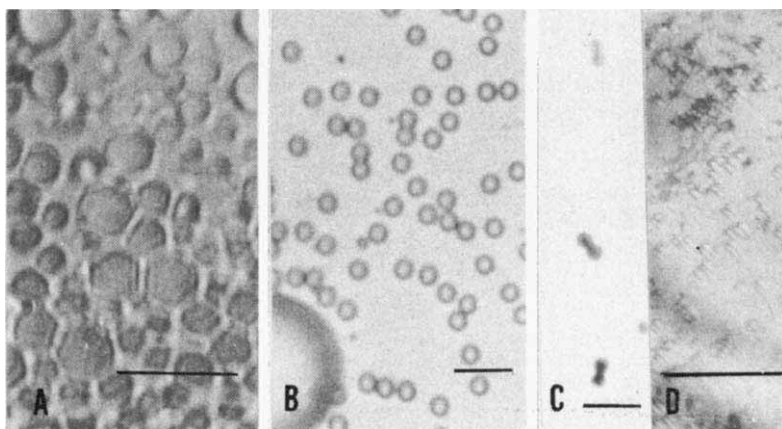


Fig. 6 *A*, Photomicrographs of the same complex microsphere in two different focal positions (*A* and *B*) showing: a darker brown core occupying the centre of the main microsphere; the presence of hemispheres and spheres of black bitumen in the main microsphere; two buds of the first generation; several colourless buds, which seem to be a second generation, originating from the first generation buds, and from the surface of the central microsphere. Althorn Farm.

Fig. 7 Size control and coupling. *A*, Unsorted microspheres in Brownian motion in water; note the development of transient buds (top) and of "polygonal pattern", of chiefly hexagonally distorted microspheres; *B*, part of large water-filled inclusion with a few large microspheres (such as the one at bottom left); a generation of about 11,000 microspheres which reveal a remarkable uniformity of diameters within the 4.2–5.0 μm range, and a third group of small microspheres which reveal a rather flat maximum at 1.6 μm diameter; the microspheres are in slight Brownian motion, note some tendency of coupling; *C*, part of inclusion in which anchored, dark brown paired microspheres predominate; *D*, part of large inclusion with elongated, orientated, and preferentially coupled light buff microspheres. Althorn Farm.



threads, tubules and microspheres (Fig. 11*B, C*). (4) Motilities and other functional aspects: it was found that beside the random Brownian motion, certain particles may rotate, and within the larger microspheres, motility of emulsified material (Fig. 4*A*) has been observed. Soft microspheres, which squeeze each other into a polygonal pattern, often show transient bud-like protrusions and a considerable degree of distortion (Fig. 7*A*). Time lapse photomicrography seems to indicate that such soft microspheres may occasionally divide and coalesce as an effect of interplay of internal and external Brownian motions, although the observations are hitherto not conclusive on account of the difficulties of keeping the rapidly moving minute particles in the focal plane of the microscope. In some inclusions (Fig. 2*A*) the particles are in oscillatory or circular movements on intensive illumination with visible light of any wavelength, but the phenomenon does not occur on heating to 200° C.

The tendency of formation of smooth hemispheres of limonite and goethite, further globular crystals and delicate threads of calcite, was observed in inclusions which were predominantly filled with oil.

There is a considerable morphological variability of particles from inclusion to inclusion of the same crystal. In spite of this, each locality does not cover the complete morphological

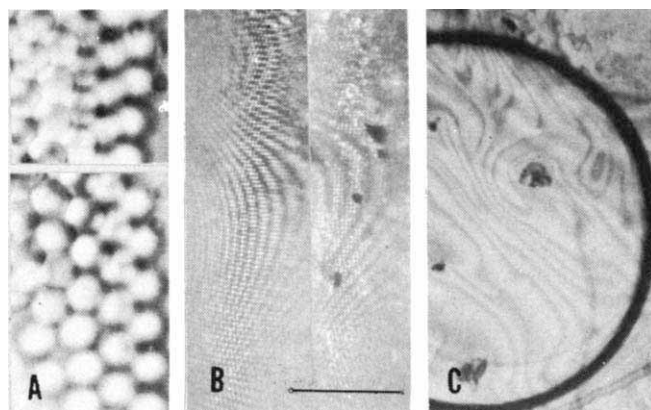


Fig. 10 Three dimensional gigantic lattices. *A*, Two portions of a three dimensional gigantic lattice of light buff-coloured microspheres; *B*, part of a moiré pattern of overlapping lattices of minute microspheres, the entire lattice being estimated to contain in each sheet 190,000 microspheres; *C*, moiré pattern presumably caused by overlapping lattices which are so fine that they cannot be resolved with the optical microscope. Althorn Farm.

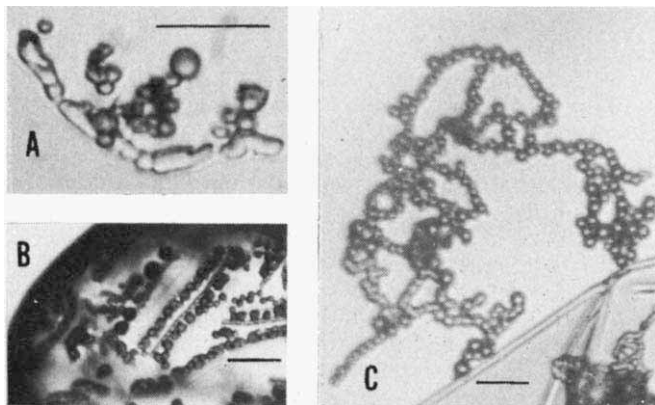


Fig. 8 Chains. *A*, Part of large water-filled inclusion with about 200 colonies of globular and ellipsoidal particles with more or less pronounced trends for chain formation; *B*, oil filled inclusion with chains of brown microspheres, note trends of zigzagging or regular successions patterns of microspheres of two distinct diameter ranges in most of the chains; *C*, water-filled inclusion with films (bottom) and chains of globules of brown bitumen. Althorn Farm.

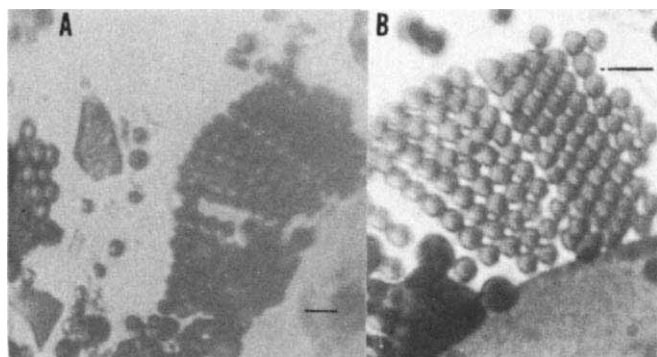


Fig. 9 Gigantic lattices. *A*, Large water-filled inclusion with films (chiefly bottom right) and microspheres of diverse shades of the brown bitumen; a hexagonal, two dimensional lattice of a generation of approximately 6 µm diameter, light buff microspheres appears on the left; another larger group with areas of quasi-hexagonal and cubic two dimensional packings of dark brown, approximately 4.5 µm diameter microspheres appears out of focus on the right; *B*, detail of the right group of microspheres of *A*. Berseba Native Reserve.

range. The spirals and spiral cracks have been observed only in specimens from Althorn Farm, whereas the polygonal patterns are almost exclusively from Geiaus and Althorn Farms. The tubule-and-globule structure has been observed mainly from crystals from the unspecified locality within the Klein Karas district. Crystals from Berseba Reserve contain the most regular gigantic lattices. The carbonaceous complex in the crystal from Gruenau contains the black bitumen mostly as fragments.

Theoretical Significance

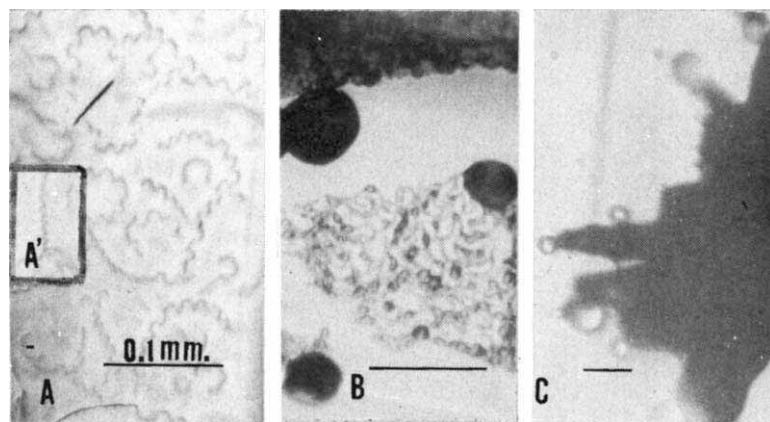
From a morphological point of view, some of the organic particles from the SW African localities closely resemble coacervate droplets^{5,6} and proteinoid microspheres^{7,8} as seen, for instance, in Figs. 1, 4 and 7*B*. The chains of microspheres (Fig. 8) have also been produced on cooling of synthetic proteinoids⁹. The gigantic lattices (Figs. 9 and 10) are comparable, both in scale and in structure, with the assemblages of liquid crystals of synthetic lipids^{10,11} which have been interpreted as representing certain advances towards biological structures. The microspectrograms of the gigantic lattices from SW Africa indicate the presence of lipids.

The organic particles from SW Africa are more pronouncedly biomorphic than the rare, acid resistant, vaguely globular objects described from carbonaceous meteorites, which may be partially organic¹². Better defined globular and hexagonal particles from carbonaceous meteorites^{12,13} have been interpreted as glassy spray particles, and limonite pseudomorphs after troilite plates, respectively¹⁴.

The SW African structures tend to resemble closely some of the objects from Precambrian sedimentary formations, which have been generally interpreted as possible fossil micro-organisms. Such structures have been described from the Gunflint formation of Canada¹⁵ and less well defined but older structures have been observed from the Onverwacht formation of S Africa¹⁶.

Hydrothermally associated bitumens from Lower Carboniferous strata of Windy Knoll, Derbyshire, England, form assemblages of several well defined organic substances with sharp phase boundaries^{17,18}, quite similar to the material within the SW African hydrothermal veins. The organic phase of the Derbyshire assemblages with optical properties within the range of that of the "brown bitumen" of SW Africa proved to be markedly enriched in amino-acids, organic acids and other biological molecules, when compared with the mean for the entire carbonaceous complex of the locality (ref. 18 and unpublished work carried out at the Institute of Molecular

Fig. 11 Some rare structures. *A*, Spiral surface cracks on rodlet of brown bitumen anchored in a water filled inclusion; *A'*, spiral structures in the interior of the rodlet, Althorn Farm; *B*, pattern of threads and spherules, Klein Karas district; *C*, pattern of brown tubules with light buff globules, Klein Karas district.



Evolution by Dr Kaoru Harada, Dr P. E. Hare, Mr Jun-ichi Oh-hashii and Mr C. R. Windsor). The results of preliminary investigations indicate that the optical equivalents or quasi-equivalents of the SW African bitumens also appear among suits of distinct organic mineraloids associated with other hydrothermal veins; for example, from the Precambrian of Besmer Mine, British Ontario, Canada; the Silurian of Middleville, New York, the Carboniferous of the Illinois-Kentucky Orefield and from several mercury veins of California, which traverse Tertiary formations. In all these localities, the brown bitumens are often present as simple microspheres, although the more complex biomorphic structures, which are characteristic for the SW African localities, have so far been found to be absent or relatively rare.

This summary seems to demonstrate that the organic particles of brown bitumen from SW Africa are more pronouncedly biomorphic than any other natural organic particle observed so far which is not a microfossil or an inferred microfossil. Some of the chemical data available at present seem to indicate that the remarkably biomorphic characteristics of the SW African particles may be interconnected with an unusually high percentage of such types of organic molecules, which form the bulk of the organism in general. Thus according to the analytical results of Dr Moore, the carbonaceous complex present in the inclusions has an N/C ratio of 0.2, which is higher than that of any hitherto analysed bitumen or oil, and it seems to be indicative for the presence of bio-organic molecules or their degradation products. The detection of isoprenoids³ and of porphyrins by Dr Hodgson seems to furnish evidence for the actual preservation of bio-organic molecules, or closely related species of molecules, within the inclusions.

The trend of concentration of lipids and substances with conjugated double bonds within the microspheres of the brown bitumen has been observed with the microspectrophotometer. The low abundance of biomorphic structures (about 0.01 to 1 p.p.m.) in material so far collected from SW Africa is a technical problem for any future work on detailed interrelations between the morphology and the molecular composition of the individual particles originating from the diverse localities.

The genesis of the organic particles can be explained by one of the two following hypotheses. (*A*) The organic substances within the veins are products of hydrothermal mobilization (distillation, melting-out and so on) of the surrounding biogenic sediments. On ascent of the hydrothermal fluids, a portion of the ultimately biogenic mobilizates would condense into the organic particles. This process seems to resemble, at least in its main aspects, the experimental procedure of Oparin^{5,6} in his production of coacervate droplets from pre-existent biological material, such as egg albumen. (*B*) The abiogenic organic substances reached the vein together with the juvenile inorganic volatiles, and the organic particles precipitated on cooling of the solutions in the course of their ascent. This process seems to resemble (when ignoring probable

differences in the chemical composition of the natural organic particles) the experimental procedure of Fox^{7,8} for the synthesis of proteinoid microspheres. According to this procedure, amino-acids (which may be abiogenic) can be condensed to proteinoids at 120–180° C, and a solution of these proteinoids in boiling water precipitates the microspheres on cooling to room temperature. It is interesting to note in this connexion that the observed homogenization temperatures of the inclusions³ and results of the present work indicate a minimum trapping temperature of about 120° to 160° C.

The hitherto inconclusive geological data on the SW African localities seem to favour hypothesis *B*. The rock which immediately surrounds the veins at Geiaus and Althorn Farms is a normal diabase, and the metarocks flanking the diabase dike seem to contain no carbonaceous matter. It seems unlikely that the organic substances, with densities mostly below unity, would have migrated downwards within the vein, from the presently eroded overlaying sediments.

These data may support the theory of Fox¹⁹ that living systems were initiated through processes similar to those of the synthesis of proteinoid microspheres, and occur in the vicinity of fumaroles and hot springs. The actual formation of biomorphic organic particles in the deep-seated vents of hydrothermal sources is clearly indicated in the SW African localities. A relatively low proportion of these organic particles became included and preserved in the crystals of the gangue minerals. The bulk of them would have been expelled through the surface opening of the hydrothermal vent, which might have been either an artesian spring or a fumarole. In the course of their dispersal throughout the soil, rivers and oceans,

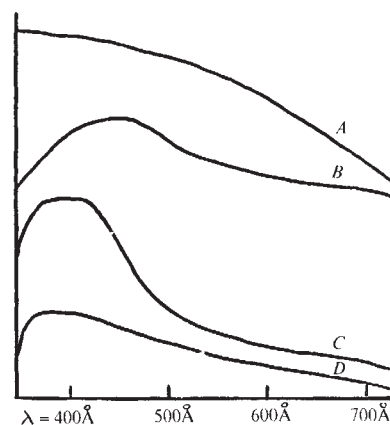


Fig. 12 Microspectrograms of four organic particles from inclusions in quartz crystals of SW African localities. *A*, Vaselineous oil, central globe of Fig. 2C; *B*, one of the small microspheres of brown bitumen in Fig. 2D; *C*, the left extreme elongated microsphere from the chain of Fig. 8A; *D*, an area of about six globules from the right central portion of the gigantic lattice of Fig. 9B. For explanation see text.

certain types of these hydrothermally produced organic particles could have undergone a second stage of evolution towards the organism.

Remote Sensor Exploration for Life

Some of the glassy particles retrieved by the Apollo 11 mission from the lunar surface have the general appearance of microorganisms²⁰⁻²², and they might have been mistaken for such in remote sensor photomicrographs²³. The particles lack, however, the more elaborate life-like morphological features and aggregations, which are exhibited in the SW African organic material. The rarity of such "SW African type" organic particles on the surface of the Earth may be explained by their being devoured by the organisms. It is conceivable, however, that on Mars, or on another celestial body to be explored in the future, the hydrothermally dispersed "SW African type" organic particles are still preserved in quantity, and they may be detected through remote sensors. Such an organic particle assemblage on Mars could be mistaken for a quasi-equivalent of a terrestrial microfauna, although some features, such as regular surface patterns, three, four, five or six-fold symmetries, would be absent. It seems, therefore, that only remote sensor data based on the detection of metabolic processes could enable us to distinguish between the equivalents or quasi-equivalents of SW African type, hydrothermal organic particles, on the one hand, and the organisms on the other hand.

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Virus Satellite Plaques

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"Satellite" plaques of a lysozymeless T4 phage can develop around plaques of phage capable of producing T4 lysozyme, which then diffuses out from the plaques.

If one developing virus plaque could aid the development of plaques of a different virus, dependent "virus satellite plaques" would be found around the nourishing plaque. This would constitute an extracellular complementation of one defective virus by another virus. This new concept should be distinguished from "helper" or "satellite" virus interactions which involve intracellular complementation¹⁻⁶. We have illustrated such a phenomenon with strains of *Escherichia coli* bacteriophage T4; and we have demonstrated a related phenomenon—virus plaques satellite about a bacterial colony—using plaques of a lysozymeless T4 mutant nourished by colonies of *Bacillus subtilis*.

Wild strains of T4 phage produce lysozyme, which ruptures

the *E. coli* cell wall late in infection and releases the mature phages. With mutants of T4 which do not produce lysozyme, propagation usually depends on the addition of egg white lysozyme or some other agent that disturbs the integrity of the cell wall. With this in mind we predicted that T4 lysozyme, diffusing out of a T4e⁺ (lysozyme producing) plaque, would nourish the development of a nearby T4e (lysozymeless) infected cell into a plaque of this latter phage.

We plated 10⁶ *E. coli* B indicator bacteria, 60 T4rIIe⁺ phage (which make large plaques when grown on an *E. coli* lawn) and 3 × 10⁴ T4IIe lysozymeless phages in top agar. The citrate-tryptone agars used were similar to those of ref. 7 but were modified to contain 2.5 mM sodium citrate, 50 mM Tris and Noble agar (Streisinger, personal communication).

After overnight incubation small satellite plaques of the lysozymeless mutant appeared, but only around the large rIIe⁺ plaques (Fig. 1).

This satellite plaque development probably proceeds as follows. The small T4 lysozyme molecules (of molecular weight 19,000, ref. 8) rapidly diffuse out of the growing rIIe⁺ plaque. That this diffusion is rapid is attested to by the presence of the lysozyme haloes well known to bacterial virologists. The lysozyme molecules then reach a cell already infected with

rIIe phage. This infected cell, containing at least a hundred rIIe phage progeny, may have its membrane already ruptured by a phage-induced membranolysin^{9,10}, but the phages are still trapped within the murein sacculus¹¹ until the sacculus can be ruptured. Autolytic murolysin present in normal *E. coli*¹¹ may occasionally allow release of lysozymeless phage but not enough to cause plaque formation. The phage lysozyme, provided by the nourishing rIIe + plaque, in the presence of citrate which damages the lipopolysaccharide barrier, attacks the murein of the lysozymeless-phage-infected cell, releasing these lysozymeless phage to start the satellite plaque growing. Cells infected with phage are probably more sensitive to lysozyme than uninfected cells, thus aiding plaque formation. Such a satellite plaque phenomenon with lysozymeless phage has also been seen by G. Streisinger (personal communication).

The *E. coli* strain used was BsuII⁺, and the T4 phages were deletion mutants in rII (r73 of Doermann¹²) and in e (eG59 from Streisinger). Other substrains of *E. coli* B and other T4 phages gave similar effects but at lower efficiencies.

Satellite plaques with lysozymeless phage should exist around any source of a murolysin able to attack *E. coli*. This would include not only other phages, but also bacterial or fungal colonies capable of releasing the proper murolysin or other wall weakening agent. We have shown this prediction to be true with an autolysin-releasing bacterial colony, *Bacillus subtilis*, as nourishing agent. Twenty *B. subtilis* ATCC 6051 cells were plated in 2.5 ml. of citrate-tryptone top agar (as above) on top of a plate of citrate-tryptone bottom agar. When this had gelled, 2.5 ml. of the citrate-tryptone agar containing 5% agar was added and the plate was incubated overnight. Then *E. coli* indicator bacteria and rIIe lysozymeless phage were plated as a soft agar overlay as before except that no rIIe⁺ phage were added. After another overnight

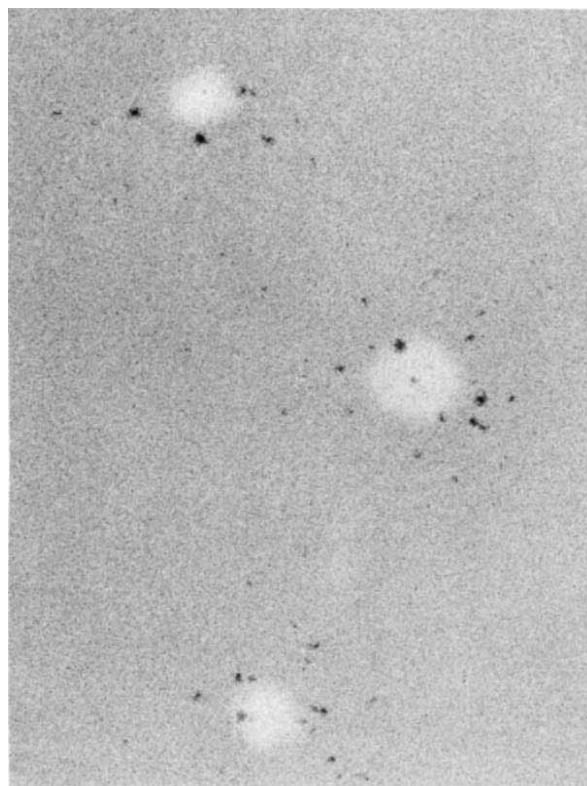


Fig. 2 A shadow picture of satellite plaques of lysozymeless T4 bacteriophage (r73e59) around large murolysin producing colonies of *B. subtilis* 6051. The *E. coli* lawn is Bsu⁺. The *B. subtilis* colonies have a diameter of 1.5–2.0 mm (see text).

incubation, plaques of rIIe phage are seen but only where nourished by the *B. subtilis* colonies (Fig. 2). Here the required murolysin has been provided by the autolysin of *B. subtilis* Marburg derived strains¹³.

Although we have illustrated virus satellite plaques with prokaryote viral systems, involving diffusible enzymes, such phenomena may exist for eukaryote viral systems and may, in any prokaryote or eukaryote viral system, involve diffusible components (enzymes, viral parts, nutrients and so on) either larger or smaller than lysozyme. A well known, comparable satellite phenomenon, with dependent bacterial colonies satellite about nourishing bacterial colonies, is seen, for example, with *Haemophilus influenzae* around *Staphylococcus aureus* colonies. This latter effect finds practical diagnostic use daily in clinical laboratories throughout the world.

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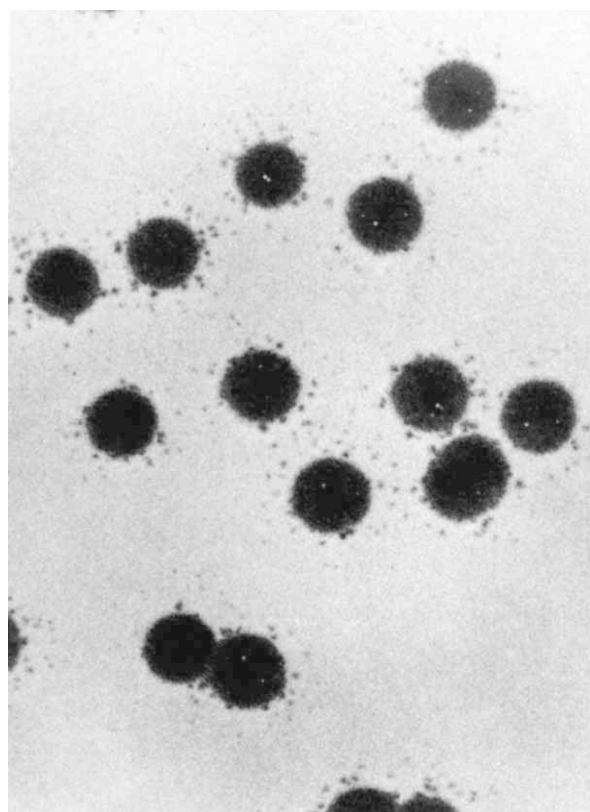


Fig. 1 A shadow picture of satellite plaques of lysozymeless T4 bacteriophage (r73e59) around large lysozyme producing plaques of T4 bacteriophage (r73) growing on a lawn of *E. coli* Bsu⁺. The small white spots in some of the plaques are colonies of *E. coli* mutants resistant to the phage. The large r73 plaques have a diameter of about 1.5 mm (see text).

LETTERS TO NATURE

PHYSICAL SCIENCES

Mechanism for the X-ray Pulsations in Cyg X-1

We wish to suggest a mechanism for producing the observed X-ray pulsations from Cyg X-1 and other sources such as Cir X-1, which exhibits similar temporal behaviour¹. We submit that the pulsations are related to oscillations in a region of high magnetic field produced by giant flare-like events. Before discussing details we briefly review the observations.

The essential temporal characteristics of the 2–10 keV emission from Cyg X-1 are as follows. First, large fluctuations of intensity exist on time scales ranging from 50 ms to 10 s or longer; individual pulses can contain up to 50% of the power; second, pulse trains with periods between 0.3 and 10 s, lasting to several tens of seconds, are observed. They can contain between 10 and 25% of the power. No single period is consistently present. The average luminosity in the 1–10 keV band is $10^{37} d_{\text{kpc}}^2 \text{ erg s}^{-1}$ where d_{kpc} is the distance to the source in kiloparsecs¹. A flux of the same order is emitted in hard X-rays (10–200 keV). The soft X-ray spectrum is steep, corresponding to an energy power law index of ~ -2.8 or a temperature $\sim 10^7 \text{ K}$. The hard X-ray spectrum is flat, corresponding to a power law ~ 0.5 with cutoff around 150 keV or a temperature $\sim 10^9 \text{ K}$ (refs. 1 and 2).

The fact that pulse trains occur at various periods which do not repeat makes it difficult to account for the temporal structure of Cyg X-1 in terms of the mechanics of large masses (rotation or pulsation). Localized activity, in analogy with solar flares, seems more plausible. Indeed, pulsations have been observed in solar flares both in hard X-ray³ and radio bands^{4,5}. These pulsations occur in pulse trains lasting $\sim$ minutes and have a period $\sim$ seconds. Pulsations at soft X-ray energies have not been observed, presumably because the cooling time for solar flare plasma is longer than the observed pulsation frequency. The most likely explanation is in terms of “ringing” of the magnetic field structure with a period

$$\tau \sim \frac{l}{v_A} \sim \frac{l}{B} \sqrt{4\pi M_p n} \quad (1)$$

where l is the characteristic size of the flare region, v_A is the Alfvén velocity, B is the magnetic field strength, M_p is the proton mass and n is the number density.

It seems that such a scheme might well explain some of the Cyg X-1 phenomena. The general picture is something like this: a flare occurs in a region of high magnetic field on the surface of a star, resulting in oscillations in the magnetic field structure. These oscillations lead to heating of the plasma in a flux tube which emits thermal radiation. The heating may be caused by magnetic pumping or particles accelerated in periodic bursts by the flare. Synchrotron radiation from high energy particles accounts for the hard X-ray emission.

If a fraction f of the 1–10 keV X-ray luminosity, L_x , is attributed to this flare, then

$$n^2 l^3 p(T) \sim f L_x \quad (2)$$

where $p(T)$ is the thermal emissivity per unit volume of thin

plasma with a temperature T . For $3 \times 10^6 \lesssim T \lesssim 10^8$, $p(T) \sim 2 \times 10^{-23} \text{ erg cm}^{-3}$ (ref. 6). The energy in the pulsations must be driven by magnetic energy, so for a pulse train of length t

$$B^2 l^3 \sim 8\pi f L_x t \quad (3)$$

Solving equations (1), (2) and (3) for the quantities l , n , and B yields

$$\begin{aligned} n &\sim 10^{16} \tau^{-6/7} (t/10)^{-3/7} d_{\text{kpc}}^{4/7} f^{2/7} \text{ cm}^{-3} \\ l &\sim 10^9 \tau^{4/7} (t/10)^{2/7} d_{\text{kpc}}^{2/7} f^{1/7} \text{ cm} \\ B &\sim 10^6 \tau^{-6/7} (t/10)^{1/14} d_{\text{kpc}}^{4/7} f^{2/7} \text{ gauss} \end{aligned} \quad (4)$$

In order for the pulse structure to stand out, both the cooling time t_c of the hot plasma and the time for photons to escape the emitting region t_{esc} must be less than τ

$$\begin{aligned} \frac{t_c}{\tau} &\sim 2 \times 10^{-2} \tau^{-1/7} (t/10)^{3/7} d_{\text{kpc}}^{4/7} f^{-2/7} < 1 \\ \frac{t_{\text{esc}}}{\tau} &\sim 2 \times 10^{-1} \tau^{-5/7} (t/10)^{1/7} d_{\text{kpc}}^{8/7} f^{4/7} < 1 \end{aligned} \quad (5)$$

Thus for $f \sim 0.1$, $d_{\text{kpc}} \sim 1$ the pulses should be broad for pulse trains with $t \gtrsim 100 \text{ s}$ and periodicities $\tau \lesssim 0.1 \text{ s}$. Detailed study of the pulse shape as a function of t , τ and f should shed light on the validity of this model.

The heating of the plasma may be understood in terms of the electric fields generated by the motion of the magnetic field. These fields should have a strength $E \lesssim v_A B/c \sim 3 \times 10^4 \text{ e.s.u.}$. This electric field will accelerate particles in a short distance which will in turn excite electrostatic fluctuations in which the electric fields $\lesssim W/e\Lambda_D$ where W is the mean thermal energy of the particles, and Λ_D is the Debye screening distance $\approx (W/4\pi ne^2)^{1/2}$. For thermal energies $\sim 1 \text{ keV}$ and the parameters given in equation (4), the applied field is less than or of the order of the induced electrostatic fields, electron “runaway” does not occur and the plasma is heated⁷. Larger applied fields would lead to a runaway acceleration of particles and the production of a non-thermal distribution function. Thus one might expect the very short pulsation periods to have a flatter, non-thermal type of spectrum. The fact that most of the soft X-ray pulses have a steep spectrum indicates that such effects are the exception rather than the rule and that acceleration to high energies is accomplished in a stochastic fashion as particles interact with the waves.

In this case the electrons will gain energy at a rate

$$\frac{d(mc^2 \gamma)}{dt} \sim e B c (v_A/c)^2 \text{ erg s}^{-1} \quad (6)$$

In the meantime, they will lose energy by synchrotron radiation in a time which is short compared with τ , which limits the maximum energy γmc^2 which an electron can attain

$$e B c (v_A/c)^2 \sim \left(\frac{d \gamma mc^2}{dt} \right)_{\text{syn}} \sim 2 \times 10^{-15} B^2 \gamma_{\text{max}}^2 \quad (7)$$

The corresponding maximum photon energy is therefore

$$\epsilon_{\text{max}} \sim 10^{-11} B \gamma_{\text{max}}^2 \sim 10^5 (v_A/c)^2 \sim 100 \text{ keV}$$

consistent with the observations.

This model for the X-ray emission from Cyg X-1 ignores the question of the ultimate source of the magnetic field energy producing the pulsations. The flares described here must occur on the surface or in the magnetosphere of a star, but the star could be a neutron star, white dwarf or any conventional star capable of generating $\geq 10^{37}$ erg s⁻¹.

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Stable Carbon Isotope Ratios within Carbonate Concretions: a Clue to Place and Time of Formation

NATURALLY occurring carbon shows considerable variability with respect to its stable isotope composition. The ¹³C/¹²C ratio in plant material (and in ancient residues) is generally roughly 2.5% lower than in the carbonate of marine limestones as a result of fractionation during photosynthesis. A summary of carbon isotope distribution and fractionation is given by Degens¹.

Some sedimentary iron carbonates have been found to be unusually rich in ¹³C. Weber *et al.*² determined the stable carbon isotopic ratios of about sixty-five diagenetic siderite nodules in Allegheny Group sediments (Pennsylvanian of central and western Pennsylvania). These were classified as marine, brackish or freshwater on the basis of combined palaeontological and textural evidence. $\delta^{13}\text{C}_{\text{PDB}}$ values for the three groups proved to be $+1.53 \pm 1.76$ (marine), $+0.54 \pm 1.98$ (brackish) and -4.75 ± 1.13 (nonmarine). These "whole concretion" values provided a basis for empirical distinction between marine and nonmarine depositional environments within the sequence studied.

Hodgson³ examined the isotopic composition of about sixty diagenetic carbonates from marine horizons and found that very few were rich in ¹³C, as might have been anticipated on the basis of the earlier work. It should be noted, however, that Hodgson's samples included many different types of concretion, several different carbonate minerals and, in certain cases, complicated post-depositional geological histories.

Another test of possible correlation between isotopic composition and environment of deposition of diagenetic carbonates has been described recently by Fritz *et al.*⁴. This study was restricted to siderite and to a sequence of Maestrichtian sediments in western Canada. Excellent palaeontological and sedimentological control was available to classify all samples as continental (fluvial to lacustrine deposition with warm temperate to tropical climate). Siderites from the Whitemud Formation gave $\delta^{13}\text{C}_{\text{PDB}}$ values ranging between -1.1 and $+7.7\text{‰}$ with a mean value of $+3.4\text{‰}$. The Edmonton Formation siderites ranged between -2.7 and $+12.8\text{‰}$ with a mean of $+8.0\text{‰}$. Fritz *et al.*⁴ concluded that bacterial processes tend to control carbonate carbon isotope ratios and that the ratios are not indicative of the depositional environment of the sediment.

These researches are to some extent in conflict or, at least, leave some questions unanswered. Previous textural and geochemical investigations of a siderite concretion from the European Carboniferous (to be reported later) have suggested that diagenetic concretion growth occurs in two distinct stages. An early period of rapid growth within essentially uncompact sediment is followed by a protracted second phase spanning the full compactional history of the enclosing sediment. It seemed logical to investigate the distribution of stable carbon isotopes within the concretion. Subsequent to this study, a much younger siderite concretion (Holocene, Atchafalaya Basin) was analysed in the same way.

Sample CHH8 was collected from a thin (4 cm) but laterally continuous layer of siderite occurring in a sequence (Westphalian A) of grey shales containing many similar siderite layers as well as numerous discrete nodules. Several kilograms were taken from a face freshly exposed by large scale quarrying operations by the Hepworth Iron Company Limited, at Hazlehead, near Penistone, England. The siderite layer occurs not far above a pyrite-rich marine shale with abundant goniatites. The sediments enclosing sample CHH8 were probably deposited from marine or brackish waters.

A second siderite concretion (38uw20C) from sediments of the Atchafalaya River Basin was kindly provided by Dr Clara Ho, Coastal Studies Institute, Louisiana State University. The sample was recovered from a depth of 25.0 m. It has the form of a small, flattened nodule with 6 cm largest diameter and 2 cm smallest (vertical) diameter. The outer "crust" is friable and yellow-brown in colour. Unlike CHH8, the recent nodule is significantly porous, especially towards its outer margin. The enclosing sediments are part of a major freshwater clay sequence⁵.

Sample CHH8 was divided into eleven subsamples as indicated in Fig. 1 (TS, small sample of "scrapings" from the very top of the nodule; BS, a similar sample from the base). Each subsample was reduced to fine powder by agate grinding. Six powdered subsamples were prepared from sample 38uw20C.

The carbonate fraction of a portion of each of the CHH8 subsamples was dissolved in disodium EDTA solution and the residues were washed, dried and weighed. Volumes of carbonate and residue respectively were calculated assuming density values of 3.9 g cm^{-3} (siderite) and 2.5 g cm^{-3} (residue). These figures permit an estimate of sediment porosity before carbonate precipitation. This assumes carbonate precipitation to replace water in pore space.

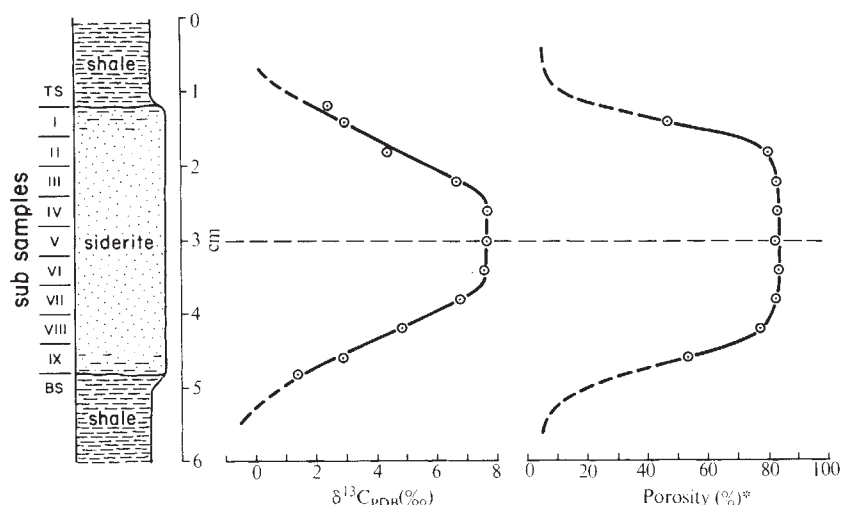
X-ray diffractometry of all subsamples showed siderite to be the only carbonate phase present. Detailed investigation of sample CHH8 (M. J. Pearson, unpublished work) showed the siderite phase to be $\text{Fe}_{0.834} \text{Mn}_{0.023} \text{Mg}_{0.091} \text{Ca}_{0.004} \text{CO}_3$ with $a = 4.71 \text{ \AA}$ and $c = 15.50 \text{ \AA}$.

Experimental procedure for isotope ratio determination followed that outlined by McCrea⁶. Finely powdered samples were reacted with 6 N HCl for 6 days. The carbon dioxide evolved was purified by passage through dry ice-acetone traps. This was then analysed by means of a nuclide 60°, 6" radius, dual collecting mass spectrometer, similar to that described by McKinney *et al.*⁷. The ratios are reported as per mil (‰) difference between the sample and the Pee Dee belemnite (PDB) standard

$$\delta^{13}\text{C}_{\text{PDB}} = \left\{ \frac{^{13}\text{C}/^{12}\text{C}_{\text{sample}}}{^{13}\text{C}/^{12}\text{C}_{\text{PDB}}} - 1 \right\} \times 1,000$$

Stable carbon isotope ratios, as ‰ $\delta^{13}\text{C}_{\text{PDB}}$, are listed for the carbonate of both concretions in Table 1. Sample 38uw20C is homogeneous with uniformly high subsample ¹³C/¹²C ratios. Sample CHH8 is homogeneous only in the central region with similarly "heavy" carbonate. Both above and below, successively lighter carbonates are encountered, and isotopic ratios close to marine bicarbonate and calcite are approached. The distribution is highly symmetrical, as can be seen in Fig. 1.

Fig. 1 Distribution of stable carbon isotopes through siderite concretion CHH8. * Porosity assumes present carbonate to occupy former pore space.



One explanation for the occurrence of unusually heavy carbonates is offered by the work of Oana and Deevey⁸, who studied the isotopic balance between all the different carbon species in a freshwater lake. They found that heavy bicarbonate (together with very light methane) is produced by certain anaerobic bacteria in fermentation reactions. Experimental work with marine anaerobic bacteria⁹ had demonstrated previously very great isotopic fractionation during methanol fermentation: $4\text{CH}_3\text{OH} \rightarrow 3\text{CH}_4 + \text{CO}_2 + 2\text{H}_2\text{O}$. Bacterial activity of this type would seem to be the only process capable of generating very heavy bicarbonate within normal sedimentary environments. Such environments would be strongly reducing: development of ^{13}C -rich siderite is logical. A similar conclusion was reached by Fritz *et al.*⁴.

The Holocene nodule seems to have developed entirely from heavy, biogenic bicarbonate. The Carboniferous concretion, however, contains significantly lighter carbonate. One interpretation of the profiles in Fig. 1 is that siderite precipitation continued throughout the compactional history of the enclosing sediments and that successively deeper (and later) diagenetic environments were characterized by lighter bicarbonate. Intensity of bacterial activity tends to diminish with depth below the sediment/water interface. The presence of marine calcium carbonates within the sediment column would provide one source of light carbonate ($\delta^{13}\text{C} \sim 0$). High ferrous iron activities would tend to stabilize siderite relative to calcite. It should be noted, however, that bacterial processes other than those discussed above would yield light bicarbonate, as would quantitative oxidation of detrital plant residues ($\delta^{13}\text{C} \sim -25\text{‰}$) by ferric oxides or hydroxides.

Independent corroboration of certain aspects of the above interpretation is offered by the distribution of clay textures and fabrics through concretion CHH8. Like "porosity" and isotope ratios (Fig. 1), the distribution is highly symmetrical.

Table 1 Stable Carbon Isotopic Composition of the Carbonate Fraction of Westphalian Siderite Concretion CHH8 and Holocene Siderite Concretion 38uw20C

CHH8		38uw20C	
Subsample	$\delta^{13}\text{C}_{\text{PDB}}\text{‰}$	Subsample	$\delta^{13}\text{C}_{\text{PDB}}\text{‰}$
Top scrape	+2.39		
I	+2.93		
II	+4.34		
III	+6.63		
IV	+7.69		
V	+7.64		
VI	+7.54		
VII	+6.73		
VIII	+4.84		
IX	+2.89		
Base scrape	+1.41		
		I	+7.38
		II	+7.26
		III	+7.49
		IV	+7.34
		V	+7.64
		VI	+7.57

Kaolinite particles within the siderite layer are more or less randomly orientated in the central region, but a definite fabric can be detected by X-ray methods towards the upper and lower concretion boundaries. This increases outwards smoothly towards that exhibited by the enclosing shale. Compactional "strains" deduced from fabric correspond with those independently deduced from variation in total carbonate content (details of this work to be published later).

Clay sediments at deposition tend to exhibit fabrics with random orientation of clay particles and porosities in excess of 70%. At a metre or two below the sediment/water interface, significant dewatering due to overburden pressure is to be anticipated¹⁰ with fabric development as clay particles rotate about axes normal to the compactional strain. Fabric development obviously will be arrested at any time of carbonate precipitation within pore space. The simplest overall picture of concretion development therefore seems reasonable. Heavy carbonate precipitated within pore space and preserved the early sediment fabric. Lighter carbonate precipitated later to include more detrital material with fabric modified by compactional strain.

It is not possible to quantify relationships between burial depth, time and fabric. The only systematic studies of concretion growth in recent sediments are those of the Atchafalaya freshwater sequence⁵. There, incipient nodule growth was noted at depths of 1 m or so in sediments not older than 20 to 30 yr. Concretions appeared to increase in size and to become more indurated with depth (observations to about 50 m). Significant compaction of the enclosing sediments was not seen at these depths. It was concluded that partial cementation of the sediment had occurred. No comparable studies of concretion growth in marine sediments are known to us. Very different pore water composition might well be anticipated to affect both concretion growth and sediment compaction. Active sulphate reduction, for example, would stabilize pyrite relative to siderite. Whereas the outer parts of carbonate layer CHH8 must have formed at very considerable depth, uncertainty remains as to the "later" time and depth limits of the early, heavy carbonate precipitation period.

We have shown that siderite concretions may be heterogeneous in isotopic composition. Where internal variation in isotope ratio is seen, it is systematic and appears to reflect total growth history. ^{13}C -rich siderites, whether formed in marine or freshwater sediments, document the activity of anaerobic, methane-producing bacteria. It is not clear how this activity is dependent on rate or composition of detrital sedimentation. On deep burial, siderite precipitation may continue but as a consequence of inorganic processes. Later-formed carbonates will be lighter and may pick up a direct environmental influence. Carbonate derived from the marine reservoir is richer in ^{13}C than that generally found in freshwater situations where there is an important contribution from soil

waters (soil microorganisms produce bicarbonate from plant residues with $\delta^{13}\text{C} \sim -25\text{‰}$).

These findings help to explain why, in spite of complex bacterial involvement, the average isotope ratio value for whole siderite concretions may provide an empirical means for differentiation between marine and nonmarine depositional environments. It does seem clear, however, that the practice of analysing whole concretions should be discontinued. The most extreme isotopic compositions are to be found in the outermost, most fissile concretionary layers. These are easily lost during collection, transport or sample "preparation".

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Upper Atmosphere Heating near the Auroral Zones

It has long been supposed¹⁻⁵ that there is a significant heat input into the upper atmosphere at high latitudes—either by Joule heating^{2,3} or by absorption of charged particles⁵. The density of the upper atmosphere certainly increases at times of geomagnetic disturbance^{6,7} and the effects seem to be enhanced^{8,9} at higher latitudes with a shorter time delay between the peak geomagnetic disturbance and the maximum density⁹. Recently DeVries¹⁰ has analysed air densities measured by the LOGACS accelerometer during the period of intense geomagnetic activity ($a_p \sim 400$) on May 25–26, 1967. The increase in density on the nightside auroral zone (crossed at ~ 2230 h LT at a height of 190 km) coincided with the onset of activity and the increase propagated to mid-latitudes after a delay of a few hours.

A unique opportunity to study the density of the upper atmosphere above about 400 km near the southern auroral zones is presented by the Russian communications satellite Molniya 1K (1968–85A). Seventeen of these satellites, in highly eccentric 12 h sub-synchronous orbits at inclinations of about 65° , have now been launched. The initial perigee height is usually about 500 km, and, in general, is severely

perturbed¹¹ by lunisolar forces because of the high eccentricity. Perigee height for Molniya 1K, however, reached a minimum several months after injection so that perigee remained near 400 km throughout 1969 and below 600 km in 1970. Because of the high eccentricity the atmospheric density is sampled over only a short arc of roughly $\pm 10^\circ$ geocentric angle. Moreover, perigee remains near the southern apex at $\sim 65^\circ$ S and each arc extends almost entirely in longitude at an almost constant latitude.

The perigee appears, relative to the Earth, alternately at one and the other of two slowly moving longitudes 180° apart. This brings the attendant difficulty that the orbit is in resonance^{12,13} with the longitude-dependent part of the Earth's gravitational field. It can be shown¹⁴, however, that the effect on Molniya-type orbits can be simply described in terms of the resonant variable

$$\Pi_{1,2} = M + 2(\Omega - \nu) \quad (1)$$

where Ω (right ascension of the ascending node) and M (mean anomaly) are two of the usual set of osculating elliptic elements $a, e, i, \Omega, \omega, M$, and ν is the sidereal hour angle. The largest contribution arises from the gravity coefficient $J_{2,2}$ which corresponds to the "ellipticity of the equator", and the resonant variable obeys the equation

$$d^2\Pi_{1,2}/dt^2 = -3n^2J_{2,2}(R/a)^2A \sin(\Pi_{1,2} + \delta - 2\varphi_{2,2} - \pi) + \text{effects of higher-order gravity coefficients, } J_{3,2} \text{ etc.} \quad (2)$$

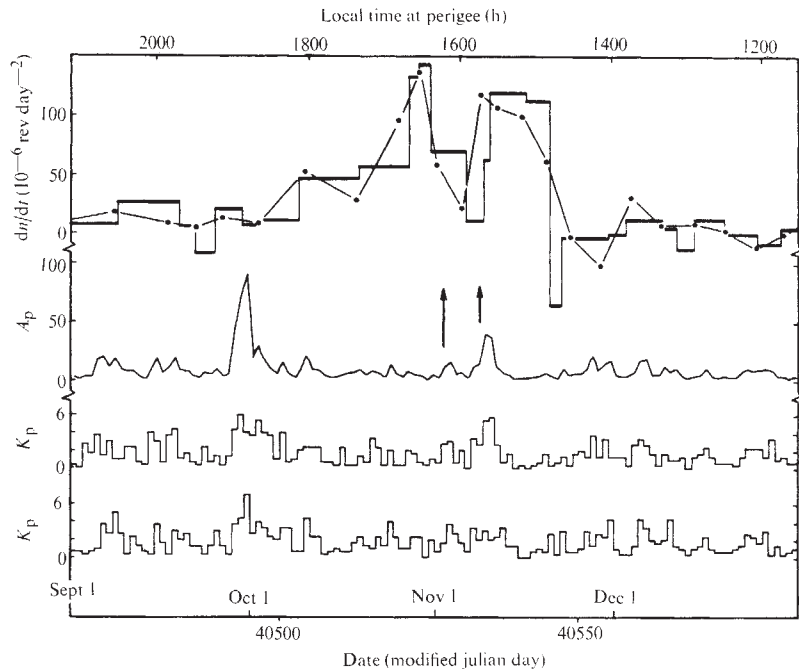
where the constants A and δ depend mainly on ω and only slightly on e and i , but the phase δ cannot exceed about 10° . In the absence of other perturbing forces, such as air drag, the ground track and the longitudes at which perigee occurs should oscillate about (very slowly moving) stable positions. In fact Molniya 1K departed very markedly from this behaviour throughout October and November 1969, when the height of perigee was $h_p \simeq 420$ km, and also slightly in December 1968 ($h_p \simeq 450$ km) and in August and September 1970 ($h_p \simeq 580$ km); the common feature is that perigee occurred near 1800 h LT. The companion final-stage rocket (1968–85D), in a similar but not quite sub-synchronous orbit, also revealed very high acceleration in October and November 1969.

Fig. 1 shows the drag acceleration dn/dt during September to December 1969—the period when the effects can be determined with some accuracy and resolution—deduced from Spacetrack data. The points joined by slant lines are derived from double differences of the resonant variable $\Pi_{1,2}$ and the data are smoothed to the extent of omitting values which give absurd swings. Similarly the histogram is based on single differences of the mean motion, n , with no values omitted. In each case the effect of the resonance forces is removed using equation (2) and taking Kaula's "combined-adjusted" solution¹⁵ for the gravity coefficients.

Both the points and the histogram show a narrow peak just before November 1, and a second broader peak from about November 10 to 20. The local time at perigee is decreasing slowly, by 2 h every 25.3 days, chiefly because of the annual motion of the Earth, and the peaks occur just after and just before 1600 h LT. The great variability, however, shows clearly that the effect cannot be a diurnal maximum due to solar EUV heating. The intense solar proton event on November 2, 1969, and the secondary maximum on November 8 fall neatly between the two peaks as shown by the arrows in Fig. 1. In any case the protons are probably much too energetic ($\lesssim 1$ MeV) to have any significant effect on the thermosphere.

Rather more surprisingly, there seems to be no significant correlation with global magnetic activity as measured by the index A_p (in Fig. 1), which was high at the end of September, but below average throughout October and November. In particular there is no response to the peak $A_p = 90$ on September 30, nor is there any better correlation with the 3 h K_p indices for the intervals in which perigee occurred; the upper K_p histogram in Fig. 1 applies for perigee near 15° E and the lower for perigee near 195° E (Table 1).

Fig. 1 Drag acceleration of Molniya 1K during September to December 1969, with geomagnetic indices. The histogram is derived from single differences of the mean motion, and the points from double differences of the resonant variable. Upper and lower K_p histograms refer to perigee near 15° E and 195° E respectively. The arrows mark the solar proton event on November 2 and the secondary maximum on November 9.



While the satellite remains Sun-stabilized, it will present almost its maximum cross-sectional area ($\sim 20\text{ m}^2$) to the atmosphere at perigee when perigee is at 0600 h or 1800 h LT and almost its minimum cross-sectional area ($\sim 7\text{ m}^2$) at 1200 h and 2400 h, and the drag will be weighted accordingly. Assuming, however, that the maximum cross-sectional area applies, the first peak in Fig. 1 gives a density of about $5 \times 10^{-14}\text{ g cm}^{-3}$ at 400 km, taking the drag coefficient as $C_D=2.2$, the mass as 1,000 kg, and assuming a scale height of 60 km. This value compares with a maximum equatorial density (at $\sim 1500\text{ h}$) of $0.76 \times 10^{-14}\text{ g cm}^{-3}$ at 400 km given by the CIRA 1965 model atmosphere for the appropriate level of solar activity $F_{10.7}=150$. In my view, even the value $5 \times 10^{-14}\text{ g cm}^{-3}$ should not be interpreted as a maximum density because it probably arises from even greater increases in density encountered only on some passages through perigee.

To improve the statistics, all individual values in each 2 h interval of local time have been combined, and the result is shown in Fig. 2 plotted with local time increasing to the right. All values have been reduced to a standard height of 400 km assuming a scale height of 100 km, a value chosen rather higher than the average scale height between 400 and 600 km according to CIRA 1965 to allow for an increased temperature at density peaks. The period covered by Fig. 1 now appears as the portion from 1200 h to 2000 h in the centre of Fig. 2.

The anomalous acceleration in August and September 1970 produced the broad maximum from 1400 h to 2000 h on the left of Fig. 2 with a peak acceleration which agrees within a factor of two with that for October and November 1969, assuming a scale height of 100 km. On the other hand the anomalous acceleration in December 1968 produces the almost insignificant peak on the right. Local time seems to be the most important parameter but magnetic local time may

be even more relevant. The discrepancy between the magnitudes of the peaks is probably not due to the different locations, presumably geomagnetic latitudes in particular, at which perigee appears relative to the Earth; Table 1 shows that these are very similar for the peaks in 1968 and 1969 but markedly different for 1970. Nor can the effect be attributed to the winter helium bulge¹⁶; instead the fact that the period of highest densities in Fig. 1 nearly coincides with the maximum of the semi-annual variation in atmospheric density¹⁷ suggests

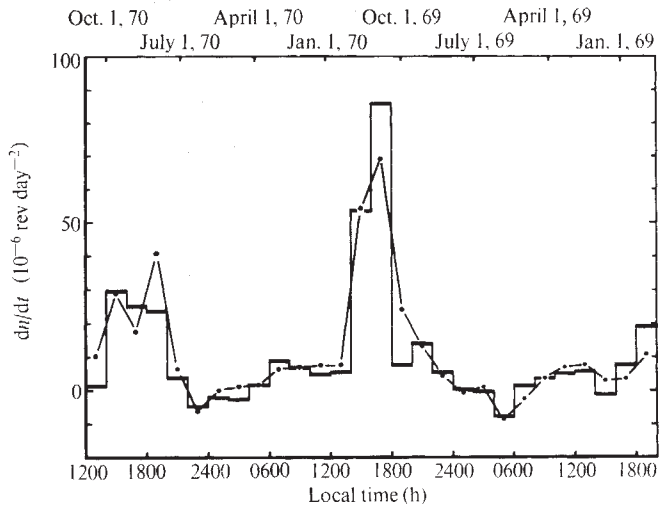


Fig. 2 Drag acceleration (two-hourly mean) against local time at perigee, adjusted to a standard height of 400 km, assuming 100 km scale height. The significance of the points and histogram is as for Fig. 1.

Table 1 Locations of Perigee at Density Peaks

	Local time	Mean perigee height (km)	Coordinates		<i>L</i> -value	Magnetic local time
			Geographic	Geomagnetic		
December 1968	$\sim 1800\text{ h}$	450	62° S, 0° E	57° S, 50° E	3.4	1600 h
			62° S, 180° E	68° S, 285° E	7.1	2100 h
October–November 1969	2000 to 1400 h	420	65° S, 15° E	60° S, 60° E	4.0	1800 to 1200 h
			65° S, 195° E	65° S, 300° E	5.6	2200 to 1600 h
August–September 1970	2000 to 1400 h	570	65° S, 40° W	52° S, 25° E	2.6	1900 to 1400 h
			65° S, 140° E	80° S, 230° E	33	2200 to 1500 h

that the two effects are related and that the auroral zone heating is enhanced near the semi-annual maxima (roughly a month after the equinoxes) and is quite possibly a principal source of the semi-annual variation.

Although the heating appears to be unrelated to global magnetic activity, it might possibly take place during moderate auroral substorms provided that they do not register significantly in the K_p or A_p indices. Substorms seem to be produced¹⁸⁻²⁰ by a contraction or collapse of the magnetospheric tail, possibly caused¹⁸ by a reversal of the vertical component of the solar wind magnetic field from north to south, which strips flux tubes from the day side magnetosphere and transports them to the tail without compressing the magnetosphere. A substorm occurs²¹ every few hours and the active phase typically lasts for 0.5 to 1 h. The heating might be accounted for either by currents flowing in the auroral electrojets round parts of the auroral oval or by the related precipitation of charged particles into the auroral zones.

Analysis²¹ of magnetograms shows that there is both an eastward current centred near 1800 h MLT, and a usually stronger westward current centred near 0400 h MLT. Cole²² estimates that the ohmic loss peaks near 150 km and that the energy input to the auroral thermosphere is $\sim 1 \text{ erg cm}^{-2} \text{ s}^{-1}$ at quiet times, rising to $\sim 100 \text{ erg cm}^{-2} \text{ s}^{-1}$ during strong disturbances. The absence of any response near 0400 h LT in the present results, however, suggests that Joule heating is not responsible.

Studies of particle precipitation have concentrated more on electrons, which are chiefly responsible for visual and radio aurorae and there seem to be two distinct precipitation regions²³. One, centred near the auroral oval at ~ 2200 h MLT, is characterized by a very variable flux with a very soft spectrum consistent with the tail population. O'Brien and Taylor⁵ suggest that the spectrum peaks near 1 keV, giving maximum heating again near 150 km. The other type²³ of electron precipitation is from the trapped population and occurs at slightly lower latitudes, chiefly in the morning sector near 0800 h. The times of occurrence of the density peaks argue against electron precipitation as the mechanism.

The present results seem more consistent with the precipitation of trapped protons which drift westward and therefore ought to be more liable to precipitate as they enter the more compressed day side of the magnetosphere. The protons might be freshly trapped either directly from the solar wind or by transferring from open to closed field lines by way of the tail, possibly during a substorm. A proton of ~ 0.5 keV energy, typical of the quiet solar wind, would probably give maximum heating above 200 km. Such a process would be expected to show a semi-annual variation if the ease of entry of the protons depends on the angle between the dipole axis and the Earth-Sun line.

The only comparable results^{24,25} are based on density gauge measurements from Explorer 32 for May to October 1966 when perigee was in the northern hemisphere. Newton and Pelz²⁴ find that the density tends to increase with latitude above 45° N and shows significant structure and that there is evidence of great variability even during geomagnetically quiet times. The effects are most marked in the highest latitude band, 55° N to 65° N , and for the local time intervals, 2000 to 2400 h, and 0000 to 0400 h. This discrepancy in time with the present results might be due to a different sampling of variations with local time and season, or simply a difference between the hemispheres. If the mechanism is the precipitation of charged particles, however, the effects ought to depend on longitude because the Earth's magnetic dipole is eccentric.

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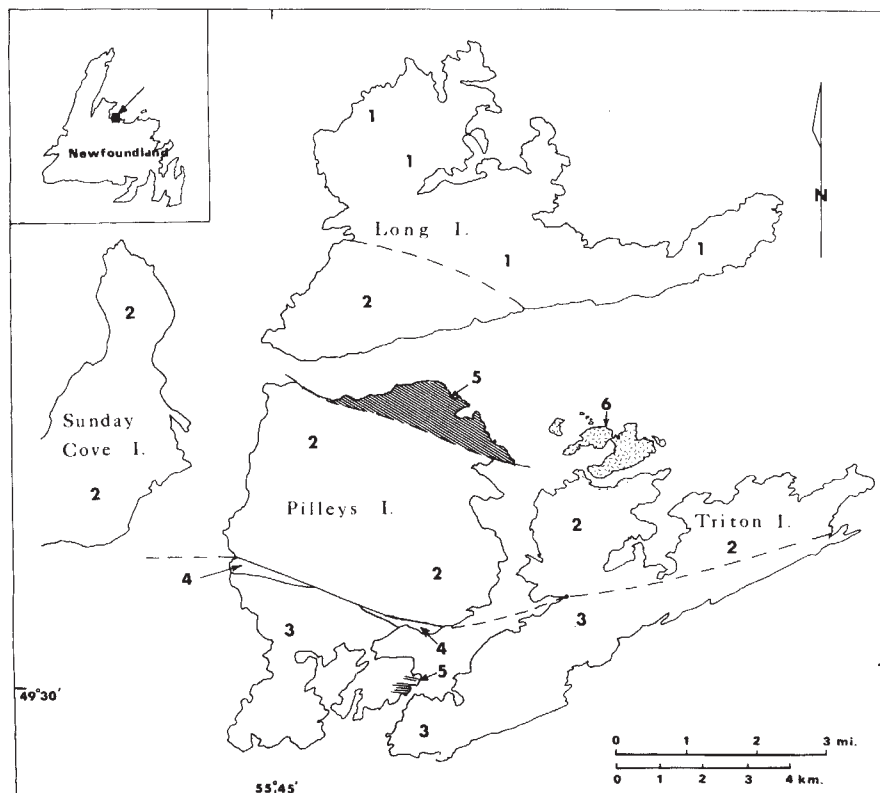
Sheeted Diabases of Central Newfoundland: New Evidence for Ordovician Seafloor Spreading

THE Notre Dame Bay area of north central Newfoundland is rapidly becoming a critical area for understanding the Appalachian mountain belt within the framework of plate tectonics¹⁻⁴. Although the Ordovician volcanic rocks in the region are of critical importance, a lack of detailed information has already led to contrasting interpretations of their origin both in ocean ridge^{1,2} and island arc³ environments.

The major geological units of the Pilley's Island area of Notre Dame Bay, as proposed by Espenshade⁵, are shown in Fig. 1. The Cutwell, Lushs Bight and Roberts Arm groups each consist of variable amounts of pillow lavas, pyroclastics and sediments, which have undergone one period of folding (B. Mean, personal communication). Evidence is strong that all are Ordovician⁶ although there has been some disagreement as to whether the Cutwell group is Lower or Middle Ordovician^{6,7}. The Roberts Arm group, generally considered younger than the Lushs Bight and Cutwell groups, differs from them in containing rhyolitic flows and pyroclastics. Espenshade⁵ mapped the Roberts Arm volcanics as overlying some 3.5 km of sediments, including both the sediments and volcanics in the "Badger Bay series". Williams, however, showed them to be in fault contact and I could not determine any definite age relations. Basal conglomerates and redbeds of the Silurian Springdale formation unconformably overlie the Roberts Arm volcanics. The Lobster Cove fault, a major regional feature, separates the Roberts Arm group and Springdale formation from the Cutwell and Lushs Bight groups.

Sheeted diabases were found in both the Lushs Bight and Roberts Arm groups (Fig. 1). Those in the former occur over an area 6 km long and 2 km wide. They consist of 100% dikes of basaltic composition and diabasic to basaltic texture. Chilled margins are visible in most of the dikes, and although contacts are generally sheared, preferred directions of chilling, such as occurs at Betts Cove⁴ or Troodos⁸, can be seen. The dikes reach a thickness of up to 14 m and average roughly 1 m, strike north to north-west and dip vertically to 80° westwards, and are in fault contact with tuffs and agglomerates of the Lushs Bight group. They are not found along strike on either Long Island or Triton Island, or in any area from which they could

Fig. 1 Geological map of the Pilley's Island area, modified after Espenshade⁵. 1, Cutwell group; 2, Lushs Bight group; 3, Roberts Arm group; 4, Springdale formation; 5, sheeted diabases; 6, Brighton (hornblende gabbro) complex.



be laterally faulted into their present position. I therefore consider them to be a vertically upfaulted block.

Sheeted diabases in the Roberts Arm group occur in two areas about 170 m apart, but they are shown as one locality in Fig. 1. The smallest occurrence is just 10 m wide, consisting of fourteen dikes averaging 0.75 m thick, only two of which display unshered chilled margins. They show local changes in dip, are highly vesicular, and pass laterally and along strike into pillow lavas. The other occurrence, roughly 170 m across strike from the above, is 50 m wide and 350 m long, bounded by and containing screens of pillow lava, and comprises dikes of $1\frac{1}{4}$ m average thickness. They vary from diabasic to basaltic in texture, with chilled margins common. I consider both of these occurrences to represent the contact zone between the top of the sheeted dike complex and the base of the Roberts Arm volcanics.

Sheeted diabases within the Lushs Bight volcanics can be

readily accepted as evidence of their origin as oceanic crust, an origin previously suggested on both tectonic^{1,2} and geochemical grounds. Those in the Roberts Arm pillow lavas are less easily explained if these lavas overlie 3.5 km of sedimentary rocks. Should this be so, the lavas could not represent oceanic crust, and the significance of sheeted diabase dikes in general would have to be reconsidered. But the lack of definite age relations between the two leads me to suggest that the sheeted diabases are evidence against the lavas overlying the sediments. This interpretation removes any basis for considering the Roberts Arm lavas as separate from those of the Lushs Bight. Furthermore, new discoveries of similar fossiliferous limestone within both the Lushs Bight and Cutwell groups strongly suggest that they are also similar. Thus, there is no significant difference in nature or origin between either the Roberts Arm, Lushs Bight or Cutwell volcanics. Any lithological differences between them are readily explained by differences in depth of water during

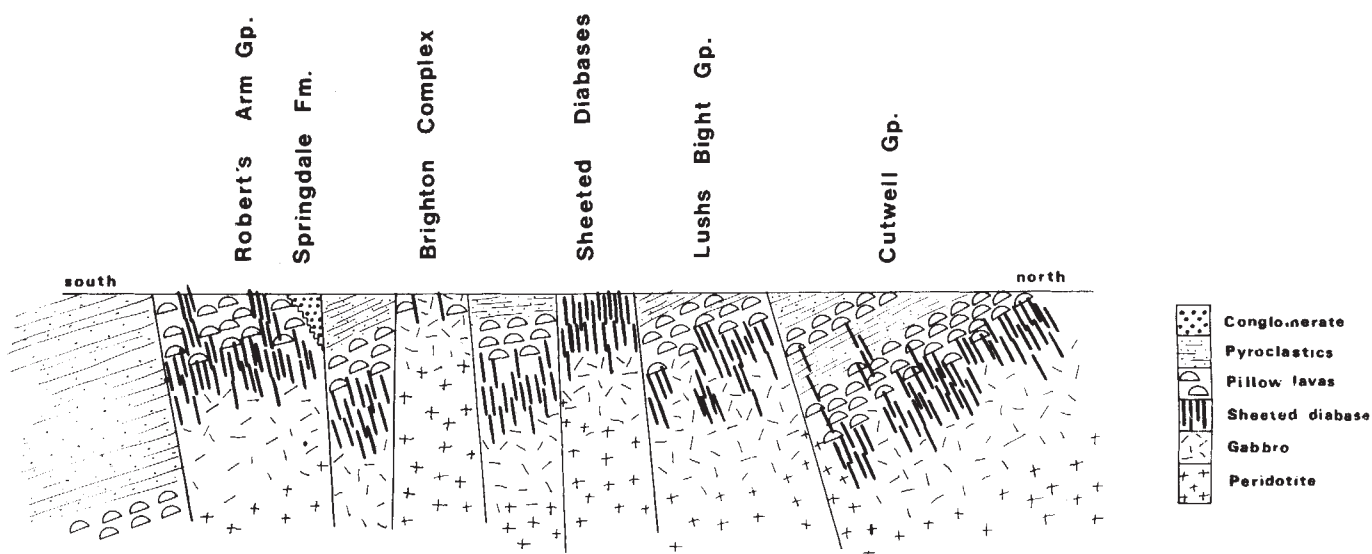


Fig. 2 Schematic north-south cross-section through the Pilley's Island area shown in Fig. 1.

their formation and by differences in level of exposure due to vertical faulting.

A model to explain the observed geological features of the Pilley's Island area is shown in Fig. 2. This model requires that the Roberts Arm, Lushs Bight and Cutwell volcanics are equivalent, requires that the Badger Bay sediments do not underlie the Roberts Arm volcanics, and requires substantial vertical faulting to expose different levels of oceanic crust. With deeper exposure one should see lower members of an ophiolite (oceanic mantle-crust) suite, that is, gabbros and peridotites, and these may be represented by the Brighton gabbro complex (Fig. 1) and small peridotite bodies which occur elsewhere in the area. Another possible implication of the vertical faulting is that the sequence is much repeated, giving an exaggerated stratigraphic thickness of the area. If further analogies can be drawn with ophiolites such as at Betts Cove or Troodos, there may be massive sulphide ore deposits occurring deeper in the sequence. In this respect, sulphide blocks and fragments are brought up in small volcanic vents throughout the area, and chalcopyrite is disseminated throughout the northern sheeted dike occurrence and in some of the pillow lavas.

Reconnaissance studies have also led to new discoveries of sheeted dikes on the eastern limits of this volcanic belt, in the Moreton's Harbour and Tizzard's Harbour areas in particular. Although their lateral extent and field relations have not yet been determined, these dikes appear to be similar in all respects to those of the Pilley's Island area. Thus, there is now little doubt that most of the volcanic rocks of the Notre Dame Bay area of the Northern Appalachians were formed by Ordovician seafloor spreading.

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BIOLOGICAL SCIENCES

Persistence of Viruses of Kuru and Creutzfeldt-Jakob Disease in Tissue Cultures of Brain Cells

THE viruses of the two spongiform viral encephalopathies of man, kuru and Creutzfeldt-Jakob disease, have been maintained *in vitro* in tissue cultures of brain cells for periods of more than 70 days and more than 250 days, respectively. Infectivity of long-maintained explant cultures of small brain fragments (and of trypsinized layers of brain cells) has been demonstrated by the transmission of the respective diseases to chimpanzees

using combined intracerebral and peripheral inoculation of the frozen and thawed cultivated cells scraped from the cultivation surface into the culture medium^{1,2}. Kuru was transmitted using an inoculum prepared from pooled brain cells from three chimpanzees with experimental kuru in third passage which were maintained as primary brain explants for 70, 215, and 238 days, respectively. Creutzfeldt-Jakob (C-J) disease was transmitted to two chimpanzees using inocula prepared from, respectively, brain cells grown *in vitro* for more than 8.5 months from a human patient and brain cells grown *in vitro* for 1 month from a chimpanzee with second passage of experimental C-J disease. Details of the inoculation and transmission are presented below. The techniques of explant cultivation of brain cells have been described previously³.

Chimpanzee A156, inoculated by intracerebral and also peripheral (intraperitoneal, intravenous, intramuscular, and subcutaneous) routes with pooled brain cells obtained from three chimpanzees and grown *in vitro* (A83, A88, A120), killed in the late stages of experimental kuru, developed kuru 13 months after inoculation. The disease was typical of experimental kuru in the chimpanzee both clinically and pathologically¹, and the incubation period of 13 months was in the range of 10 to 17 months, which we have learned to expect from experimental kuru beyond first passage in the chimpanzee. The explanted and long-maintained brain cells which produced the disease have been cultivated as primary explants with repeated changes of culture medium for periods of more than 70 days: those of chimpanzee A83 for 215 days with twelve fluid changes; of chimpanzee A88 for 238 days with twenty fluid changes; and of chimpanzee A120 for 70 days with six fluid changes. Each such change of nutrient medium, or "feeding", was accomplished by pipetting off the supernatant fluid and adding 3.0 ml. of fresh medium, to produce an approximately 1:10 dilution of the previous medium. Thus, accumulative dilution factors of 10^{-12} , 10^{-20} , and 10^{-6} , respectively, had been attained during the months of tissue culture maintenance. All three animals represented fourth passage of kuru in the chimpanzee of the virus derived from the human kuru patient EIRU. All had received brain tissue from third passage in the chimpanzee A57 and had developed disease with 14, 12, and 12 months incubation, respectively. The transmission to chimpanzee A156 demonstrated that the kuru agent had persisted for 70 days or more in brain cells which have been maintained at 37° C in the incubator. Whether virus replications had occurred during this period, or whether the agent had only persisted in those cells which have been repeatedly dividing, or in cells which had remained in a resting stage, cannot be said.

Creutzfeldt-Jakob disease has been transmitted to a chimpanzee (A154) using intracerebral and peripheral inoculation of brain cells from chimpanzee A121 maintained in explant culture for 1 month. Chimpanzee A121 had developed disease from second passage in the chimpanzee of the C-J agent obtained originally from the brain biopsy of patient R. R. in England². The disease had appeared in the first passage in 13 months (A54) and in the second passage in 14 months.

C-J disease has also been experimentally transmitted to another chimpanzee (A146) with 16 months incubation, using human C-J disease brain cells, obtained at autopsy and maintained in explant cultures for 8.5 months (255 days). The growth medium was changed fourteen times, at about 18 day intervals, during this period. Finally, the cells were scraped into suspension, then sedimented at 10,000 r.p.m. for 30 min, resuspended in one-sixth the original volume of culture medium, and frozen and thawed three times to prepare inoculum. The typical C-J disease which developed in the inoculated animal was clinically similar to that which had developed in another chimpanzee (A123) 11 months after primary intracerebral and intravenous inoculation of 10% suspension of freshly frozen human brain tissue from the same patient, and easily distinguishable from kuru.

These transmissions of kuru to one and of C-J disease to two chimpanzees demonstrate that the viruses of both diseases

have been maintained in viable brain cells in explant cultures at 37° C for periods of a minimum of 70 days and of 255 days, respectively. Although persistence of virus infectivity in these cultures is proved, active replication of the viruses, which presumably is occurring, cannot yet be established until repeated subculture of the primary explant cultures is performed and infectivity titres are carried out on these successive subcultures. Knowledge that the viruses in brain cells *in vitro* remain infectious and virulent for several months provides reason for continued study of the biochemical and immunological properties of such virus infected cells in the hope that a marker for these viruses, other than the transmission of the respective diseases to chimpanzees^{1,2} or to New World monkeys³, may be found.

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Is a Specific Protein Responsible for the Supercoiling of Polyoma DNA?

POLYOMA and SV40 DNAs contain enough genetic information to code for half a dozen proteins. Very little is known about these proteins, although their recognition is of great interest in view of the oncogenic potential of the genomes. Recently, however, polyoma DNA from productively infected cells was isolated in the form of a protein-DNA complex distinct from the virus¹ (our work in preparation). In an attempt to explore the nature and function of its protein moiety, we have investigated the effect of various inhibitors of protein synthesis on the formation and properties of this complex. We report here the results we obtained with puromycin.

Fig. 1 Sedimentation properties of DOC-extracted material and DNA in neutral sucrose solution. Twenty-eight hours after infection with polyoma virus, mouse cells were exposed for 60 min to 4×10^{-4} M or no puromycin before being pulse-labelled with tritiated thymidine (see Table 1). After another 150 min, the cells were lysed with DOC. Some of the DOC-extracted material was mixed with ¹⁴C-labelled viral DNA and centrifuged in a 5–20% neutral sucrose gradient³ for 120 min at 60,000 r.p.m. in the SB405 rotor of a B60 International ultracentrifuge (left). Some of the DOC-extracted material was treated with sodium sarcosinate (0.25%) and pronase (100 µg/ml.) for 4 h at 37° C before being mixed with the same marker and centrifuged under the same conditions but for 192 min instead of 120 min (right). Since identical radioactive profiles were produced by the marker in both tubes, the sedimentation patterns obtained for the ³H-labelled material extracted from the uninhibited cultures (●) and from the inhibited cultures (○) were superimposed. The arrows on the figure indicate the position in the gradient of the 20S covalently closed form (component I) and of the 16S open form (component II) of polyoma DNA.

Table 1 Inhibition of Polyoma Virus DNA Synthesis by Puromycin

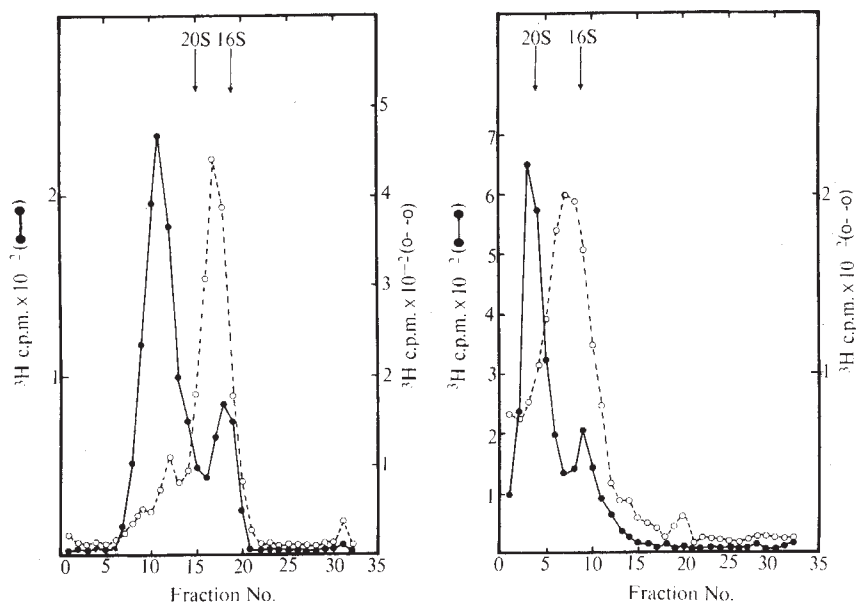
Time of radioactive pulse	Acid precipitable radioactivity in DOC extract (c.p.m. $\times 10^{-1}$ per plate)				
	0–30 min	30–60 min	60–90 min	90–120 min	240–270 min
With puromycin	1,572	1,150	719	594	349
Without puromycin	3,088		3,659		3,701

Petri dishes (50 mm) containing 2×10^6 mouse cells maintained in low serum medium¹⁴ were infected for 1 h at 37° C with polyoma virus (50 p.f.u./cell). Twenty-eight hours later (time – 30 min) the medium was replaced by 2 ml. of Herpes buffered medium² containing either 4×10^{-4} M or no puromycin (Sigma Chemical Company). At zero time and various times thereafter 0.2 ml. of the same medium containing 50 µCi/ml. of ³H-thymidine (New England Nuclear Corp; specific activity 20 Ci/mmol) was added to pairs of plates. The cells were lysed in the plate (using Na deoxycholate⁴) 30 min after the addition of the isotope and the lysates were clarified by centrifugation at 30,000g for 30 min⁴. The table shows the TCA-precipitable radioactivity in the supernatant. This radioactivity would be twenty times inferior in supernatants from similarly treated uninfected cells.

Secondary cultures of whole mouse embryos² were used to propagate the TSP 1 variant³ of polyoma virus and to synthesize the radioactively labelled viral DNA. Viral DNA was extracted from the cells in the form of a protein-DNA complex using 0.25% sodium deoxycholate (DOC)⁴. This method was found to extract from the cells more than 90% of the viral DNA (P. B., unpublished).

When cells actively replicating polyoma DNA are exposed to puromycin, a sharp reduction in the incorporation of tritiated thymidine into DOC-extractable DNA ensues (Table 1). The very rapid action of the inhibitor is remarkable in view of the fact that most of the proteins (namely polynucleotidic ligase⁵) which would be required for polyoma DNA synthesis are expected to be already present in large excess in the uninfected cell. Such an effect on polyoma⁶ or SV40^{7,8} DNA synthesis has already been observed for cycloheximide. Our findings therefore further support the idea that synthesis of the DNA of small oncogenic viruses would be dependent on the parallel synthesis of specific protein(s).

The labelled materials extracted with DOC from the untreated and from the puromycin-treated cultures have strikingly different sedimentation properties in neutral sucrose solution (Fig. 1, left): while most of the former material sediments ahead of component I marker at approximately 25S, the latter forms a band at a position intermediate between those of component I and component II markers. The relatively



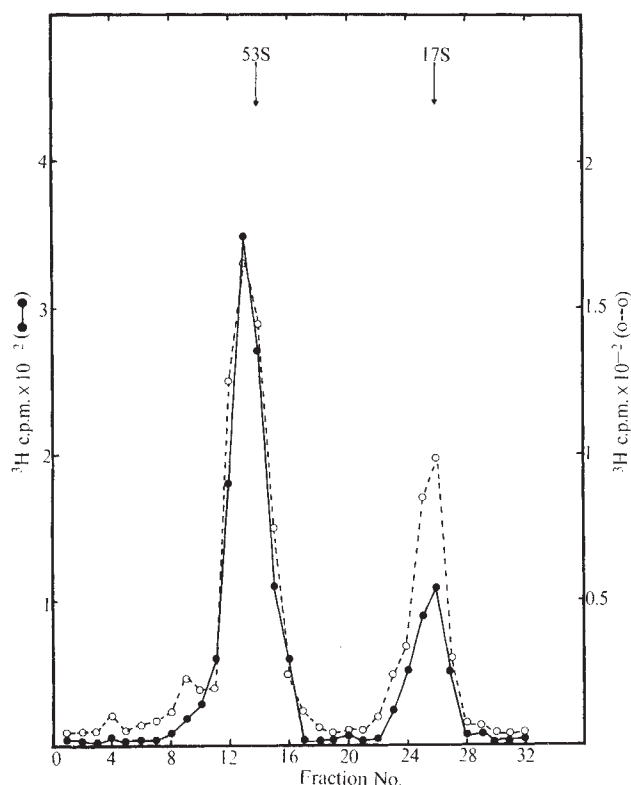


Fig. 2 Sedimentation properties of DOC-extracted DNA in alkaline sucrose solution. The same DNA as analysed in neutral sucrose solution in Fig. 1 was mixed with ^{14}C -labelled marker viral DNA and centrifuged in a 5–20% alkaline sucrose gradient⁵ for 120 min at 60,000 r.p.m. in the SB405 rotor (see Fig. 1). The radioactive profiles recorded for the ^3H -labelled DNA from untreated (●) and treated (○) cultures are superimposed. The position in the gradients of the ^{14}C -labelled denatured components I (53S) and II (17S) is indicated on the figure.

high S value of the former material is due to the presence of protein(s) bound to the DNA: after combined treatment with pronase and sodium sarcosinate—or with other proteolytic enzymes or detergents (P. B., unpublished work)—it co-sediments with component I marker in neutral sucrose solution (Fig. 1, right). In contrast, the same treatment exerts little or no change on the sedimentation properties of the material extracted from inhibited cells (Fig. 1, right). This

means that the “puromycin” DNA is not bound to protein as “normal” DNA is, or else that protein does not have the same configurational effects on each DNA.

Although reminiscent of the sedimentation properties of polyoma DNA synthesized in the presence of cycloheximide⁶ the fact that the “puromycin” DNA sedimented differently from both components I and II in neutral solution was intriguing. We therefore investigated the sedimentation properties of both “normal” and “puromycin” DNAs in denaturation conditions. In alkaline sucrose solutions, most of the labelled material from both preparations sedimented at 53S (Fig. 2), as expected from denatured component I (ref. 9). When compared, the results of the neutral and alkaline runs suggested that the “puromycin” DNA might consist mostly of covalently closed circular molecules deficient in tertiary turns.

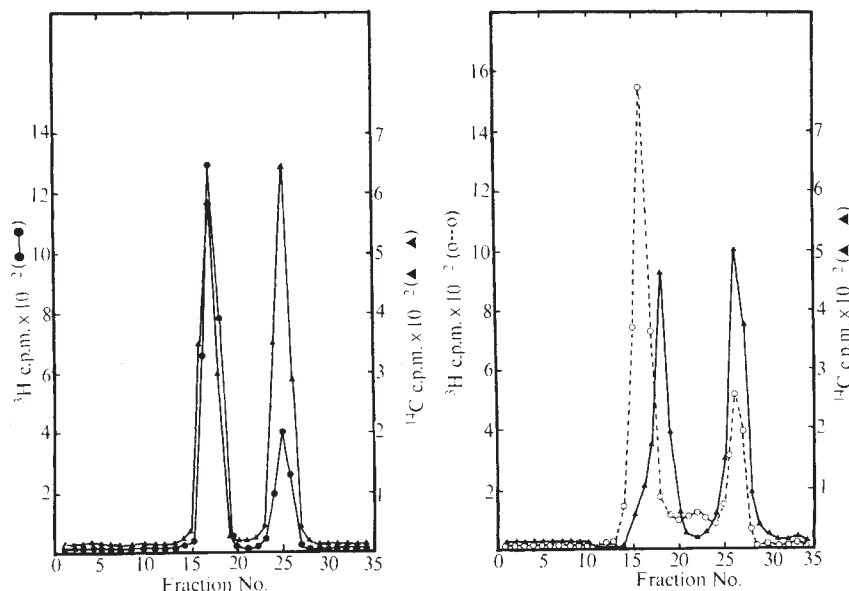
Both “normal” and “puromycin” DNAs were mixed with ^{14}C -labelled marker viral DNA and subjected to dye buoyant density gradient centrifugation¹⁰ in solutions of caesium chloride (CsCl) containing either ethidium bromide (EtBr)¹⁰ or propidium diiodide (PDI)^{11,12}. While “normal” DNA produced two radioactive bands which did coincide with those produced by the marker, the lower band formed by the “puromycin” DNA was recovered at a considerably higher density than the lower band generated by the marker (Fig. 3). This means that the covalently closed molecules present in the “puromycin” DNA have a lower superhelix density than those in the “normal” DNA¹². Since in our experiments the distance between the lower bands was always less than 2 mm, we calculated the difference in superhelix density ($\Delta\sigma_0$) between “puromycin” DNA and “normal” DNA using the Eason and Vinograd equation¹²:

$$\Delta\sigma_0 = 0.10 [(\Delta r/\Delta r^*) - 1]$$

where Δr and Δr^* represent the distance between the lower and upper bands for “puromycin” DNA and “normal” DNA respectively. A value of 0.025 was obtained. Assuming that the superhelix density (σ_0) of intracellular polyoma DNA would be close to that of SV40 intracellular DNA or virus DNA, which are equal to -0.030 and -0.039 (ref. 12) respectively, “puromycin” covalently closed polyoma DNA would contain very few superhelical turns indeed. This was not unexpected since this DNA was found to sediment slightly faster than marker component II in neutral sucrose solution (see Fig. 1).

Infected cells which had been pulse-labelled with ^3H -thymidine during puromycin inhibition were subjected to a 3 h chase in the presence of either thymidine or 5-bromodeoxyuridine: it was concluded from the analysis of the viral DNA that the

Fig. 3 Density of DOC-extracted DNA in PDI-CsCl density gradients. After treatment with sodium sarcosinate and pronase, the same DNA as analysed in Fig. 1 was mixed with ^{14}C -labelled marker viral DNA and centrifuged in 3 ml. of a solution (in 0.02 M EDTA–0.02 M Tris-HCl, pH 8.6) of CsCl ($\rho = 1.52 \text{ g ml}^{-1}$) containing PDI (500 $\mu\text{g ml}^{-1}$) for 40 h at 20°C and 30,000 r.p.m. in the SB283 rotor of a B60 International ultracentrifuge. The radioactive profiles obtained for the DNA from untreated (●), left, and puromycin-treated (○), right, cell and for marker viral DNA (▲) are shown on the figure. The marker was obtained by treating with sodium sarcosinate and pronase the DNA extracted with DOC from mouse cells which had been labelled continuously with ^{14}C -thymidine (0.5 $\mu\text{Ci/ml}$; specific activity, 43.5 mCi/mmol) from the eighteenth to the fortieth hour after infection with polyoma virus.



labelled molecules which had regained superhelical turns during the chase were not more numerous than those which had replicated. We therefore found no evidence supporting a mechanism whereby superhelical turns would be introduced in polyoma DNA independently from replication.

The effects of puromycin on polyoma DNA biosynthesis which we have just described could be due to the inhibition of the synthesis of several proteins. At least one of these, however, should be a protein which affects the configuration of the viral DNA, and possibly forms a complex with it. Whether this protein would either act by neutralizing the charges of the DNA¹³ or have enzymatic activity (twistase?) is not clear at present.

Experiments undertaken with the aim of identifying the puromycin-sensitive factor(s) with the product(s) of recognized genes of polyoma virus using temperature-sensitive mutants are in progress.

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Somatomedin: Proposed Designation for Sulphation Factor

THE action of growth hormone (GH) on skeletal tissue was proposed to be mediated through a secondary substance, described by the operational term "sulphation factor"¹. The observations underlying this hypothesis have been amply confirmed and extended. A GH-dependent plasma factor stimulates in cartilage not only the incorporation of sulphate into chondroitin sulphate, but also the incorporation of thymidine into DNA², proline into the hydroxyproline of collagen³ and uridine into RNA⁴. GH has substantially no *in vitro* effect on cartilage metabolism.

In plasma sulphation factor (SF) circulates associated with the large molecular components but extracts of considerably smaller size with sulphation factor activity have been prepared by denaturing plasma protein by boiling⁴ or by extraction with acid ethanol⁵. Van Wyk *et al.*⁵ purified their acid ethanol extracts by gel filtration, ion exchange chromatography and electrophoresis. After an approximate 25,000-fold purification, the biological activity could be attributed to a neutral peptide with a molecular weight of about 8,000 (ref. 6).

The spectrum of biological action of SF has been extended

by experiments with partially purified SF. Hall and Uthne⁷ demonstrated that injections of SF active extracts induced widening of the tibial epiphyseal cartilage. The biological actions of SF are not limited to cartilage. Salmon and DuVall⁸ showed that partially purified SF had insulin-like actions on isolated rat diaphragm and Hall and Uthne⁷ have shown that similar SF preparations stimulate conversion of ¹⁴C-glucose to ¹⁴CO₂ by rat adipose tissue. The insulin-like activity cannot be neutralized by anti-insulin serum and follows SF activity through multiple fractionation procedures⁵, suggesting that SF is identical with or very similar to the smaller molecular weight component of the non-suppressible insulin-like activity (NSILA-S) extensively studied by Jakob, Hauri and Froesch⁹. NSILA-S stimulates thymidine incorporation in rat costal cartilage¹⁰. Recently Salmon and Hosse¹¹ reported that a serum fraction with SF activity stimulated HeLa cell growth.

Although the mechanism of production and ultimate physiological role of SF remain to be defined, its importance in the growth and anabolic responses of both skeletal and certain non-skeletal tissues can be in no doubt. After consideration of many alternatives to the operational terms "sulphation factor" or "thymidine factor", we propose the more general term, "somatomedin"; the prefix, "somato", connotes both a hormonal relationship to somatotropin and, also, to the soma which is the target tissue of this agent. "Medin" is included in the name to indicate that it is an intermediary in somatotropin action.

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Repellent Odour in Aphids

We have found that *Myzus persicae* aphids are repelled by the odour of droplets released from the cornicles and squashed aphids of the same species.

Apterous adults of the green peach aphid, *M. persicae* (Sulzer), were reared on radish seedlings, and cornicle droplets were produced by applying slight pressure with forceps to the thorax. The droplets remained on the ends of the cornicles and the aphid was placed on a detached leaf with feeding *M. persicae* (adults and young), which abruptly stopped feeding and rapidly moved away from the droplets by leaving the leaf or moving to the other side. Aphids waved their antennae before and during movement. When the odour was placed near aphids feeding on the underside of leaves they fell off. These responses were also noted when fresh droplets on filter paper were brought near feeding aphids (Fig. 1). Alate as well as apterous forms of aphid were repelled by the secretion, but control, feeding aphids did not respond to dry or water-wet filter paper.

Table 1 Cross Test of Alarm Odour

Name of squashed or secreting aphid	Peach	Rasp- berry	Name of responding aphid *	Straw- berry	Willow	Pea	Cabbage
Peach	+	+	+	—	—	+	—
Raspberry	+	+	+	—	—	—	+ ?
Strawberry	+	+	+	NT	—	—	NT
Willow	—	+	NT	+	NT	NT	NT
Pea	+ ?	+ ?	+	NT	NT	+	NT
Cabbage	—	NT	NT	NT	NT	NT	—

+, Response noted, three or more aphids in a cluster moved; NT, not tested; + ?, only one or two, or none at all in some tests, aphids moved; —, no response noted.

* Common name of aphid	Latin name	Reared on
Green peach	<i>Myzus persicae</i> (Sulzer)	Radish
Raspberry	<i>Amphorophora agathonica</i> (Hottes)	Raspberry
Strawberry	<i>Chaetosiphon fragaefolii</i> (Cockerell)	Strawberry
Willow	<i>Tuberolachnus salignus</i> (Gmel.) †	Willow
Pea	<i>Acyrtosiphon pisum</i> (Harris)	Pea
Cabbage	<i>Brevicoryne brassicae</i> (L.)	Broccoli

† *T. salignus* tested individually.

Similar tests with other species gave positive responses (Table 1). In all cases except one, each species responded to its own factor but not all species responded with equal intensity. *M. persicae* and *A. agathonica* reacted violently and often dropped from the leaf, *A. pisum* and *C. fragaefolii* moved more slowly from the source of the factor, and *T. salignus* lifted its hind legs and moved a short distance around the stem.

Inter-specific results suggest that the same chemicals are found in species giving a positive response, but are present in different quantities or proportions. The waxy, white covering of cabbage aphids may obviate a chemical defence mechanism.

Whether the odour has an adaptive function and whether the only method of using it is by secreting a cornicle droplet is uncertain. Most likely it is present in the cornicle fluid reservoirs, but the negative results of tests with other body parts may indicate that an insufficient quantity of material was collected (unpublished observation). Although the chemical may function as an alarm odour, it could also be released when the aphid is consumed by a predator.

The most cited theory of cornicle function concerns defence. It is based on the rapid hardening of released liquid cornicle droplets. This hardening renders predators harmless by "fixing" them on contact¹. Edwards² observed the fixing of predators both in nature and in the laboratory, but other workers question this theory^{3,4}. Older theories of function such as excretion and respiration are discussed by Hottes¹.

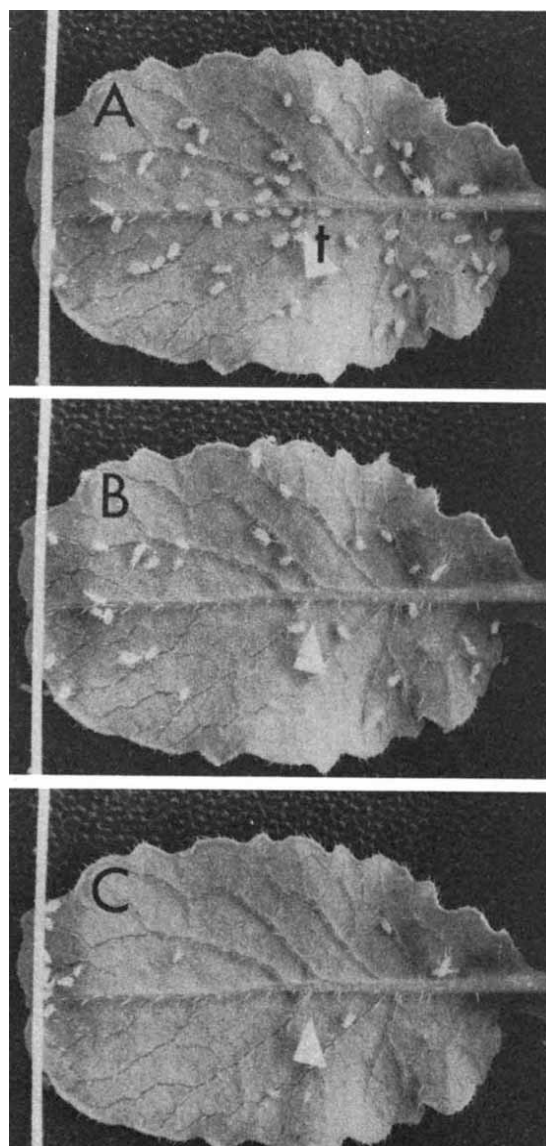


Fig. 1 Dispersal of the green peach aphid, *Myzus persicae* (Sulzer). The triangle of filter paper (t) containing the cornicle droplets was placed at A initiating the behavioural sequence: antennal movement, cessation of feeding and dispersal from the site. B and C show the same leaf at 1 and 5 min, respectively. The white line along the left side of the pictures is the edge of a 'Plexiglas' strip used to hold down the leaf. The number of aphids present in A and remaining in B and C are fifty-six, thirty-two and four, respectively.

Results of the intra-specific and inter-specific tests allowed the formulation of the hypotheses presented in Table 2. If excretory (hypothesis D), cornicles might occur in other groups of insects and the release of droplets would be periodic. Self-defence (hypothesis C-1) is not attractive as aphids do not always release cornicle wax when attacked by predators², and predators are not observed to be repelled. Cornicle size is not correlated with the presence of the odour and, furthermore, responding willow aphids have smaller cornicles than cabbage aphids, which neither respond to, nor produce, an odour. Production of droplets at the end of a long stalk would be an adaptive advantage (in the fixing theory) for placing droplets on the predator. The reduced cornicles in responsive aphids suggest that the fixing hypothesis is not an adequate explanation for the function of cornicles in all species.

Hypothesis A-2 is not attractive as aphids do not should not be repelled when alien aphids are being repelled.

C-2 is attractive, even though aphids rarely secrete droplets when attacked. A predator would release the odour and in

Table 2 Hypotheses for the Function of Cornicle Factor

I Slow release—intermittent, partial opening of cornicle valve to release small amounts of volatile factor. No droplet visible.	
(A) Territorial—	(1) intra-specific spacing of aphids. (2) prevent influx of other species.
(B) Dispersal—	Induce migration in aphids to the new growth of the plant.
II Fast release—visible droplet released.	
(C) Defence—	(1) Self-individual aphid repels a predator. (2) Social—individual aphid's release of factor when attacked by a predator causes other aphids to disperse or drop from the leaf.
(D) Excretory—	Release of metabolic matter or toxic plant compounds.

addition contaminate its mouthparts when eating an aphid. Hypothesis *B* is attractive, also, because emerging radish leaves will have a high density of fairly mature *M. persicae*, which migrated from other regions of the plant. Aphids on older leaves may sense the emergence of new leaves and signal for dispersal.

A-1 is attractive even though the intermittent, partial opening of the cornicle valve has not been observed. *M. persicae* are grouped near the base of the cotyledons on radish plants in a fraction of the space available to them, but are dispersed over much of the new leaf surfaces (Fig. 1A). Initial attempts to extract the active factor indicate that some difficulty may be expected with a bioassay as ether, one of the solvents employed, repels aphids.

The presence of a repellent or alarm odour in aphids is not unexpected, as many other species in the closely related Hemiptera possess alarm substances⁵. The significance of this odour in the natural biology of the aphid is unknown; it is not known either whether the spread of plant viruses for which aphids act as vectors would be enhanced or diminished by using the odour to control aphid infestations on economic plants.

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Stimulation of Partial Limb Regeneration in Rats

INJURY^{1,2}, nerves³ and hormones⁴ have been identified as essential for limb regeneration in amphibia. There seem to be differences in the "current of injury" between regenerating and non-regenerating types of amphibians⁵; partial limb regeneration can be induced in the latter type by simulating the "current of injury" of the regenerating form⁶. The cellular process of fracture healing in the amphibian is directly related to the electrical phenomena produced by the fractured bone⁷ and maximally effective ranges for current density can be determined at the cellular level. This led to the concept of a control system the key element of which was the induction of

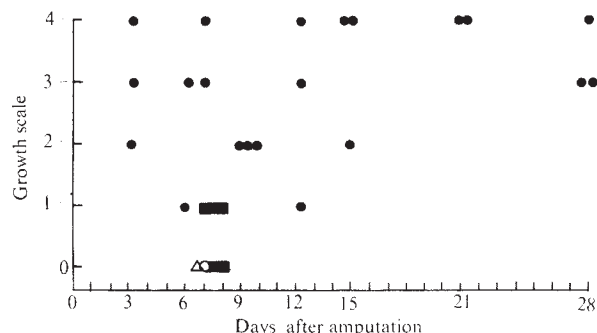


Fig. 1 Graphic representation of the results of all animals, control and experimental. The growth scale is as described in the text. ●, experimental series (twenty-two animals); ○, control series A (eight animals); ■, control series B (seven animals); △, control series C (two animals).

blastema formation in response to appropriate electrical factors⁸. The absence of regeneration in the mammal may therefore be due to the absence of adequate electrical factors. We report here the consequences of restoring factors which seem to be of practical as well as theoretical interest.

Measurements of the current produced by Smith's simple silver-platinum bimetallic couplings in Ringer solution indicated levels (5–15 nA) in excess of the optimal range determined for cellular stimulation in fracture healing. The insertion of a miniature 10 M ohm resistor at the junction of the silver and platinum wires resulted in a measured current closer to the desired level (3–6 nA). Several simple modifications of the basic device were manufactured to produce currents ranging from below to above the desired level and tested in a series of 21 day old, male, Sprague-Dawley rats. The experiment included three control series (A, B, C) and one experimental series with thirty-nine animals. All surgery was done under ether anaesthesia with aseptic precautions, each animal receiving 0.2 ml. of a long acting antibiotic (procaine penicillin, Pfizer) subcutaneously at the end of the surgical procedure. No hormonal supplement was used. Control series A consisted of eight rats with a guillotine type amputation through the right foreleg at the junction of the mid and lower thirds of the humerus (proximal to the distal epiphyseal plate) without the implantation of any device. Series B consisted of seven animals similarly amputated with the immediate implantation of a low current (0.001–0.1 nA) metallic device consisting of either two short lengths of No. 28 AWG silver soldered together (two animals), two similar sections of platinum wire (two animals) or a single length of silver wire (three animals). The solder joints and wires were encapsulated in medical grade silicon (Silastic-Dow Corning) except for 2–3 mm exposed wire at both ends. These (and all subsequent devices) were inserted beneath the remaining limb musculature, adjacent to the residual humeral shaft, with the distal end bent and inserted 2–3 mm into the medullary cavity. The proximal end was brought out through the shoulder muscles in the vicinity of the greater tuberosity and sutured in place to the superficial muscle fascia. Control series C consisted of two animals similarly amputated with the immediate implantation of a high current (5–15 nA) silver-platinum bimetallic coupling with no interposed resistor, similarly encapsulated with silicone. Twenty-two animals were used for the experimental series in which a device consisting of a silver-platinum coupling joined through a 10 M/ohm resistor and similarly encapsulated was inserted in the same fashion at the same amputation site. In all animals of this series, the platinum electrode was distal. All animals were maintained on standard laboratory rat diet with water *ad lib.* until they were killed by etherization. All control series were killed 7 days after amputation, while the experimental series were killed at intervals ranging from 3 to 28 days. No infections were noted in any animal of any series and no animals were discarded from any series. In each case, the implanted device was removed,

the residual upper extremity (humerus with attached musculature) was removed *en bloc*, fixed in buffered formalin, prepared in the usual manner, serially sectioned longitudinally and stained with haematoxylin and eosin. Growth was assessed on a scale of 0–4+, 0 being that consistently seen following simple amputation, 1+ any growth in excess of control but not as a recognizable blastema, 2+ presence of a recognizable blastema, 3+ a blastema with the differentiation of a specific tissue type such as cartilage, bone or muscle within the blastema and 4+ the appearance of an organized multi-tissue structure with anatomical characteristics appropriate to the morphological area.

Fig. 1 summarizes the results. Simple amputations without implantation of any device resulted in some subperiosteal osteogenesis and bridging of the cut surface with new bone and formation of a simple fibrous cap (Fig. 2A). No control animals with very low current devices (control series B) demonstrated growth in excess of 1+, while the animals with high current devices (control series C) demonstrated bone destruction and cyst formation (Fig. 2F). In the experimental group, only two animals were as low as 1+ with the remainder higher than this, including eight instances of 4+ growth. Seven of the experimental animals were killed at or before the seventh post-amputation day and may thus be directly compared with all

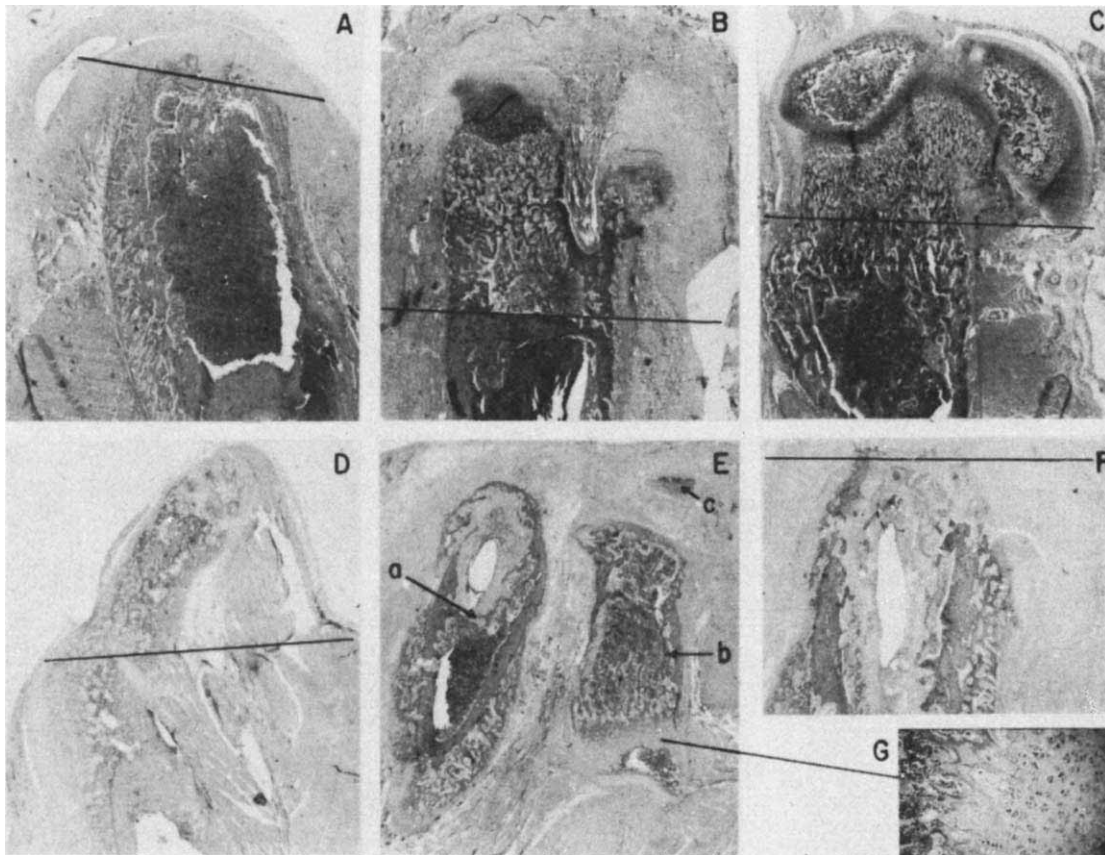


Fig. 2 Low power ($\times 10$, unless otherwise noted) photomicrographs of longitudinal sections through the amputation site. All sections were stained with haematoxylin and eosin and the horizontal dark line indicates the approximate original level of amputation. All sections, except G, are oriented with the distal portion uppermost. The level of the original amputation was determined either by new bone which was clearly distinguishable from the old bone or by angulated or eccentric new growth on the original shaft. *A*, Control rat (series A, no implanted device) 7 days after amputation. The subperiosteal new bone growth and the tendency to bridge across the cut surface of the humerus are evident. *B*, Experimental rat (implanted device with 10 M ohm resistor) 14 days after amputation. Considerable growth in length has occurred with a cancellous bony shaft and two cartilaginous growth centres distally. New muscle formation was noted in this specimen distal to the cartilaginous "epiphyses". *C*, Experimental rat (implanted device with 10 M ohm resistor) 3 days after amputation. Again, considerable new bone growth has occurred, though less than in *B*. The distal portions have formed two well developed epiphyses with a well organized epiphyseal plate proximally and normal appearing joint type cartilage distally. The epiphyseal centres contain borders of cancellous bone and centres with apparently normal haematopoietic marrow. The 50% offset of the new growth on the shaft is apparent, the deviation occurring towards the side of the humerus to which the device was attached. *D*, Experimental rat (implanted device with 10 M ohm resistor), 7 days after amputation. A long "finger" of cancellous bone has grown from the original resection area. The distal portion of the growth is a reasonably well organized single epiphysis with a small centre of ossification and a proximal epiphyseal plate. This growth also was eccentric on the shaft and the residual shaft of the humerus is represented here by the tangential section visible at the lower border of the photomicrograph. The cancellous bone between this and the resection line is actually the normal subperiosteal bone growth sectioned tangentially. *E*, The same animal as in *D*, showing the supernumerary limb forming adjacent to the humeral shaft. This growth was also eccentric to the midline of the original shaft which is here represented by the tangential section (*a*). The supernumerary humerus (*b*) is well formed with a proximal head epiphysis, a well organized epiphyseal plate (*G*), a bony diaphysis containing normal appearing haematopoietic marrow and a distal cartilaginous plate. Distal to this structure is a zone of transversely organized fibrous tissue followed by a small osteo-cartilaginous structure (*c*) which is taken to represent the anlage for either radius or ulna. *F*, Control rat (series C, implanted device with no resistor). The bony resorption back from the original amputation level is particularly evident on the right side of the shaft in the photomicrograph as is the reactive cyst that formed near the site of the electrode implanted in the medullary cavity. *G*, Higher power ($\times 100$) view of the proximal epiphyseal plate observed in the supernumerary humerus (*E*). This figure is rotated 90° from its corresponding placement in *E* to show the longitudinal organization of the epiphyseal plate. In this figure the right hand portion represents the proximal portion of the epiphyseal plate and the left hand portion is distal, closest to the bony diaphysis.

control groups. In this portion of the experimental group only one demonstrated 1+ growth, one 2+ growth, three 3+ and two 4+. In the remainder of the experimental group that went for periods longer than 7 days, a higher percentage of 4+ growth was noted. The commonest form of 4+ growth consisted of regrowth of the distal humerus complete with two epiphyseal centres. The majority were organized as shown in Fig. 2B, one was structurally indistinguishable from the normal except that it was laterally displaced by approximately half the diameter of the humeral shaft (Fig. 2C). In one case the longitudinal growth was strongly curved and contained only one epiphyseal centre (Fig. 2D). A larger than normal blastema, however, had formed in this case with a large mass extending proximally along the humeral shaft. Developing within this mass was a complete supernumerary humerus (Fig. 2E) with appropriate morphological appearance including a proximal epiphyseal plate (Fig. 2G), head epiphysis, bony diaphysis with a medullary cavity containing apparently normal haematopoietic marrow, a distal cartilaginous surface and an osteo-cartilaginous anlage of another bony structure distal to this. In all animals exhibiting 3+ or 4+ growth, there was formation of new, organized skeletal muscle distal to the transection line accompanied by ingrowth of nerves and blood vessels.

Studitsky⁹ reported that rats could regenerate a single minced skeletal muscle. Carlson^{10,11} has confirmed the original report and extended the histological observations. Whether the process is truly regenerative is questionable, for no recognizable blastema appears at any stage. In a few cases, osteo-cartilaginous structures appeared within the muscle mass, arising apparently from fragments of the Achilles tendon (Carlson, personal communication). The rat otherwise is capable of only a small measure of muscle regeneration at the histological level¹². Moreover, the regeneration of organized multi-tissue extremity structures is lost in the rat after the fourteenth day of gestation¹³. The only report of successful restoration of rudimentary limb regeneration in any mammal is that of Mizell¹⁴ who implanted nerve tissue into the hind limb of the newborn opossum before amputation. The opossum, however, is born at a very early stage of development with particularly rudimentary hind limbs, and these results may be interpreted as enhancement of foetal type regeneration.

The results of the present study are interpreted as indicating support for the original thesis⁸ that regenerative growth could be restored in mammals by the application of appropriate levels of electrical stimulation to produce the cellular changes necessary for blastema formation. The origin of the blastema formed in these experiments is unknown. Further experiments will be necessary to find the effect of simultaneous hormone (prolactin) administration, the most effective range and method of electrical stimulation, the efficiency of the method in other mammals including those in older age groups, and possible undesirable side effects.

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Lead Levels in Deciduous Teeth of Urban and Suburban American Children

LEAD poisoning is a serious threat to the health of American children living in urban ghettos. Perlstein and Attala, studying 425 children treated for lead poisoning, found mental retardation in 22%, recurrent seizures in 20%, and cerebral palsy in 2%¹. Many more children eat lead-containing substances and bear raised body levels of lead, but are not identified as being intoxicated². If children with elevated tissue lead could be reliably identified, and their neurological and psychological function evaluated, the incidence of silent brain damage from lead ingestion could be established. Because elevations in blood lead are transitory, and decline once ingestion has stopped, blood lead levels are unsatisfactory indices of earlier exposure. Calcified tissues, however, store lead: bone biopsy procedures are impractical for large scale surveys, but it occurred to us that the exfoliated primary tooth might offer a way to examine the lead content of children who are at risk. Alshuler *et al.* reported a substantial increase in the lead content of deciduous teeth of children with fatal and treated plumbism³.

Sixty-nine deciduous teeth were collected from dental clinics serving children who resided in the "lead belt" of urban Philadelphia, the area of the inner city from which most cases of lead poisoning are reported. Forty teeth were obtained from suburban dentists, where lead poisoning and, by inference, lead ingestion are largely unknown. The external surfaces of the individual teeth were cleansed with a detergent solution, then caries, filling materials, and adherent soft tissue manually removed. The teeth were rinsed, then crushed to a fine powder in a 'Freezermill' (Spex Industries) at -196° C. The powder was mixed with 5 ml. of bromoform (specific gravity 2.85), exposed to ultrasonication for 15 s (30 W, maximum tuning) and centrifuged at 3,000g for 5 min. This procedure removed any residual particles of dental amalgam. A further density separation was performed at specific gravity 1.6 to remove carious tissue. Seventy per cent perchloric acid (2 ml.) was then added to the dried tooth powder to dissolve the inorganic components of the tooth and precipitate the dental proteins. The protein precipitate was washed twice with 1 ml. aliquots of 0.2 M perchloric acid and the washings were combined with the acid-soluble portion of the tooth. The perchloric acid was buffered with 1.0 ml. 0.2 M glycine-HCl (pH 2.6) and the pH adjusted to pH 2.2-2.8. The lead was chelated with 1 ml. 1% ammonium pyrolydine dithiocarbamate and the lead complex was extracted into 1 ml. of 2,4-dimethyl-6-heptanone. The diphasic solution was centrifuged at 3,000g for 5 min and the organic phase removed. The concentration of lead in the organic phase was determined on an EEL 140 atomic absorption spectrophotometer.

Fig. 1 shows the differential distribution of lead in children from the "lead belt" and from the suburbs. The mean tooth lead for suburban controls is 11.1 ± 14.8 p.p.m., and for children from the ghetto 51.1 ± 109.0 p.p.m. After the data were normalized by logarithmic transformation, because of the skewed distribution, a *t*-test was applied ($P < 0.01$). One child,

identified by history as having been treated for lead poisoning, had a tooth lead level of 110 p.p.m. While eleven children in the suburban group had tooth lead levels of 2 p.p.m. or less, four children had levels too low to be detected by the atomic absorption technique. No urban children showed levels of lead below 2 p.p.m. This finding may be due to differences in atmospheric lead in the city and the suburbs. The data show that in areas where lead eating by children is frequent, tooth lead levels are elevated. This finding suggests that the deciduous tooth can be used as an indicator of lead intake, and possibly that of other trace elements, in children. Studies are now under way to examine neurological and psychological function in subjects with elevated tooth lead levels.

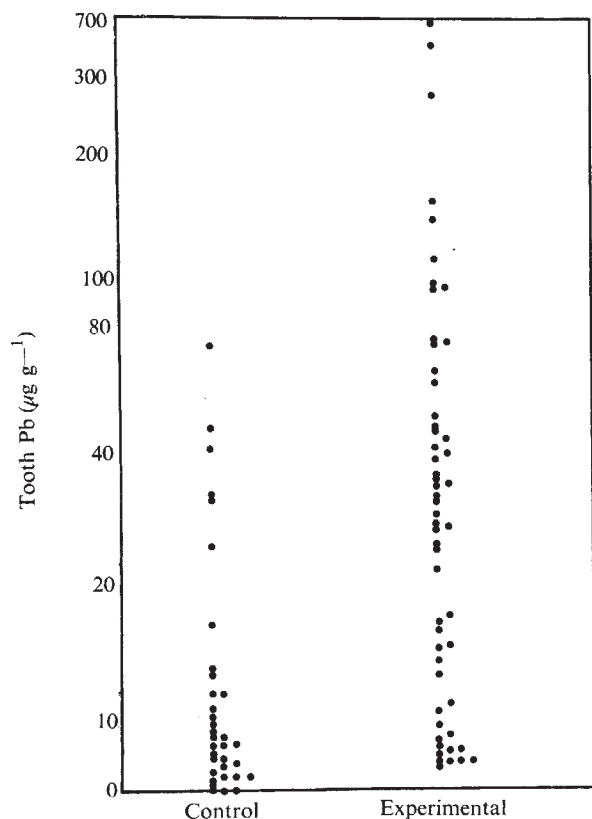


Fig. 1 Tooth lead levels of urban (experimental) and suburban (control) children.

Assessment of Marginal Malnutrition

Kanawati and McLaren have proposed that the ratio of mid-arm circumference to head circumference is a measure of marginal malnutrition¹. Midarm circumference can indeed be used as a rapid and cheap indicator of severe protein-calorie malnutrition (PCM)^{2,3} if measurements can be compared with standards for children of the same age. But in many areas parents do not know the precise age of their children. In such cases, arm muscles mass, which reflects body protein status, may have to be appraised in community studies by comparison with body tissues which reflect age but which are less affected by PCM. These ratios are less precise than arm circumference related to age because the normal growth rates of the indirect parameters differ from that of arm circumference. They are also affected by nutritional status to some extent.

Kanawati and McLaren report a strong correlation between arm circumference/head circumference ratio and the "index of thriving", but since the "index of thriving" also contains arm circumference and head circumference this is to be expected. They also report a good correlation between weight for age and the proposed ratio. But, given a large population and substantial differences between normal and clinical conditions, a good correlation would be obtained when there are few or no intermediate cases, precisely the group of most interest. The question is not whether a good correlation exists between population means but rather, given the variation in normal populations, does the ratio found in marginal malnutrition differ significantly from normal?

Inadvertently, they interpret a tissue test that we have used incorrectly^{4,5}; we reported that the mean bulb diameters of each of the depressed weight for age groups were significantly smaller ($P < 0.01$) than those of the healthy controls and that the mean bulb diameters of each of the moderate malnutrition groups were significantly larger ($P < 0.01$) than those of clinical cases. On the basis of diameter measurements alone we could not separate the three weight for age classifications of mild to moderate PCM from each other.

The authors also maintain that epilation is traumatic and that laboratory equipment is required. In fact, children rarely show distress during the procedure. We feel that it is the most rapid tissue test for field work, the total sample time being about one minute. The only equipment needed is a pair of forceps and an envelope in which to store the samples. Unlike many biological specimens there is no need for immediate stabilization procedures and the samples do not need to be refrigerated. In the laboratory, the only equipment needed to examine specimens is a low power microscope. Hair tissue preparations afford the opportunity to screen a population at three levels in the PCM sequence: as an index of early malnutrition (reduction in normal bulb diameter), of severe PCM (increase in bulb atrophy), and of chronicity (anagen-telogen shift). (A technique manual is now in the press.)

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BOOK REVIEWS

Nicolaus Steno as Geologist

Dissertations on Steno as Geologist. Edited by Gustav Scherz. Vol. 23. *Acta Historica Scientiarum Naturalium et Medicinalium*, Copenhagen University. Pp. 319. (Odense University: Odense, 1971.) \$11.70.

THE remarkable career of the seventeenth century Danish anatomist, geologist and Catholic bishop Niels Stensen (commonly known as Nicolaus Steno) and his contributions to science have formed the subject of a considerable number of scholarly studies in the past twenty-five years or so, especially by, or at the suggestion of, Dr Gustav Scherz, the editor of this book. Those who knew Gustav Scherz will learn with regret, from a note inserted in this volume, that while passing the final proofs of the book he died, at the age of 76, as the result of a road accident. In his preface Dr Scherz records the death of his friend, the Danish historian of geology, Axel Garboe, who also published some important works on Steno which perhaps tend to be overlooked. A reference to the life and work of Dr Scherz and to some of the earlier works of these authors may not be out of place here.

For many years discussions of Steno's original and important contributions to the foundation of modern geology mentioned only his well known "Prodromus to a Dissertation concerning Solids naturally contained within Solids", the title of the earliest English version of his chief geological work, first published in Latin in 1669¹. Steno's earliest geological ideas were, however, incorporated in an earlier work, an account of the dissection of a shark, which forms an appendix to an anatomical treatise on the muscles that he published in 1667. We are indebted to Dr Garboe for making this historically important geological work better known, for he made a translation into English, the first in any modern language, in 1958². The original is of considerable rarity, as are all of Steno's works, and not easily accessible to most readers.

The circumstances that led Dr Scherz to undertake his detailed studies of Steno's life and work are mentioned in the brief account of his life inserted in his last work by Professor A. Noe-Nygaard. Scherz was born in Austria

and ordained as a Catholic priest in 1919. He moved to Denmark in 1922 for health reasons, and later obtained Danish citizenship. It was here that he first became aware of the career of Steno, who had been converted to Catholicism while in Italy. His studies were at first centred on Steno's theological works, published during the latter part of his life, after he had abandoned science to become an ascetic priest and bishop. To these studies, later extended to cover Steno's scientific works, Scherz devoted the last thirty years of his life.

In 1954 a commission of theologians and historians was established to set in motion the process which, it was hoped, would lead to the beatification of Steno. Scherz became joint editor of a periodical *Stenoniana Catholica*, published to promote this end. By this and other means he did much to make Steno's life and work more widely known.

In pursuing his studies Scherz searched many European libraries and archives, and brought to light much unpublished matter bearing on Steno's life and activities. This included many letters written by or to Steno, among his correspondents being a considerable number of well known seventeenth century scientists. Scherz also traced some unpublished manuscripts in Steno's hand, and arranged for their publication and translation. One of his most important publications is his two volume edition of Steno's correspondence, which contains a great deal of biographical and bibliographical matter relating to the persons named in the correspondence³.

Scherz's single minded enthusiasm for the subject of his researches was such that he was able to persuade other historians of science to cooperate with him in producing translations of works by Steno and critical articles on various aspects of his work. This last volume of his contains thirteen articles, the work of twelve authors. They deal mainly, though not exclusively, with Steno's geological work, seven being written in German, five in English and one in Italian. Scherz himself has contributed the longest article, on Steno's extensive travels through much of Europe, which demonstrate that he had ample and varied opportunities of studying geology in the field. Articles on Steno's use of actualistic methods in geology; on Steno and geological time;

on Steno's significance in the foundation of geology and palaeontology; and on Steno as the founder of the geology of Tuscany, have been contributed, respectively, by Professors K. von Bülow of Rostock, T. Frängsmyr of Uppsala, H. Hölder of Münster and F. Rodolico of Florence. Articles discussing the part played by Steno in the founding of crystallographic science have been contributed by Professors J. G. Burke of Los Angeles, I. I. Schafraanovskii of Leningrad and C. J. Schneer of New Hampshire. A short but interesting article by S. Springer of the US National Museum of Washington, DC, discusses the geological deductions made by Steno after dissecting a shark's head, when he concluded that *glossopetrae* (fossil shark's teeth) really were organic in origin and not, as generally supposed hitherto, sports of nature.

One of the attractive features of many of the works published by Dr Scherz, including that noticed here, is their lavish illustration by means of contemporary portraits of European scientists known to or mentioned by Steno, and views of the places visited in his travels. This volume should not be confused with a previous one in the same series entitled *Steno's Geological Papers*, published in 1969. It is worth recalling that the latter contains an exhaustive bibliography of Steno's geological works, reprints and translations of these works, and of critical articles discussing his contributions to geology.

In the preface to the work reviewed Scherz states it was intended as a sort of literary symposium to mark the tercentenary of the publication of the *Prodromus*. In fact, it has appeared in the year that marks the tercentenary of the publication in 1671 of the first translation of the *Prodromus* into English. Historians of science and geologists particularly owe a debt of gratitude to Father Scherz for his labours in making Steno's contributions to geological science better known and more accessible.

V. A. EYLES

¹ Stenonis, Nicolai, *De Solido intra Solidum Naturaliter Contento Dissertationis Prodromus* (Florence, 1669).

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Photo-ecology

Aerial Photo-ecology. By John A. Howard. Pp. xvii+325. (Faber and Faber: London, November 1971.) £7.

THIS book is intended to provide a compact source of information and data on the application of air photography to ecology, integrated land resource studies and forestry. It is not a successor to Spur's *Photogrammetry and Photo-interpretation* and it illustrates the difficulties, even for an experienced worker, of writing an account of a subject which embraces several disciplines, each of which is undergoing extremely rapid development.

The main sub-divisions are "Factors influencing the aerial photograph", "Elements of aerial photography", "Elements of photogrammetry", "Basic interpretation and integrated interpretation", each section consisting of two or more chapters. The chapters of the first section discuss the camera, the film, tropospheric conditions and the reflectance properties of vegetation. Minor omissions are unavoidable, but the cursory treatment of a topic such as colour film is unexpected. The descriptions of negative and reversal colour film are compressed into a few lines, there is no mention of subtractive colour, or any explanation of the colours which are seen in the infrared colour film and, contrary to what is stated, it is possible to obtain colour prints from colour film positives without a negative step but such prints, and particularly Cibachrome, are expensive working material. The almost total absence of illustrations in colour is disappointing and the collo-type reproduction of the stereo pairs and of many of the illustrations is inadequate for a book of this type.

A delay in publication of three years detracts from the value of the book not merely in that the costs of aerial photography in the second section are quoted in shillings and pence (d) but much more serious is the absence of any reference later than 1966, with the result that there is, for example, no mention of Gaussman's work in the chapter on the reflectance of light by vegetation, or any reference to Dawkin's recent work on the tropical forest in the last section.

The section on the "Elements of Photogrammetry" provides a useful introduction to this subject although one wonders for whom the description of such techniques as the slotted template is intended.

The section, "Basic Interpretation", contains useful practical advice but both this and the last section would have been enhanced by being illustrated with well reproduced stereo pairs. The absence of any mention of microdensitometry in a discussion of tone difference is surprising. The use of the density scale and its relevance

to the second of the two apparently unrelated keys to African vegetation is not explained.

The last section, on "Integrated Interpretation", covers a variety of topics; the two chapters on forestry surveys under tropical and temperate conditions are probably the best of the six chapters. Other chapters on geography, geology and geomorphology are rather uneven though they will serve to make methods such as land system analysis more widely known.

The account of survey design in the chapter on regional biological survey proved very difficult to follow. (Sampling the effective area of every photograph using the effective area as defined earlier in photogrammetric terms would seem to involve sampling the same area twice.)

This book will probably not come up to the expectations of many of those who have awaited its appearance for so long. It is to be regretted that the first hand observations of the author tend to be lost in the welter of diverse topics.

A. BLAIR RAINS

Attack

The Physiology of Aggression and Defeat. Edited by B. E. Eleftheriou and J. P. Scott. Pp xi+312. (Plenum: New York and London, 1971.) £6.30.

The Imperial Animal. By Lionel Tiger and Robin Fox. Pp. xi+308. (Holt, Rinehart and Winston: New York, September 1971.) \$6.95.

THE first of these books (a record of a symposium held in 1968) describes experimental work on the social interactions of mammals. It seems to have been printed from uncorrected typescript; there are many minor errors. The other book is evidently intended to be popular. The two have little common ground.

In the symposium, a paper by F. H. Bronson and C. Desjardins is entitled "Steroid Hormones and Aggressive Behavior in Mammals". (The title, like others in this volume, suggests a wider coverage than is actually attempted.) It briefly reviews the literature on effects of hormones on attack and allied behaviour. It also describes the authors' studies of laboratory mice. Males are first put in isolation for some weeks; this makes attack on other mice more likely ("isolation-induced aggression"). The influence of hormones on this behaviour is then studied. These authors include observations on a sensitive period in mouse development: hormonal treatment just after birth can profoundly influence later capacity for intolerant social behaviour.

B. E. Eleftheriou gives a preliminary account of "Effects of Aggression and

Defeat on Brain Macromolecules". He too uses male laboratory mice which have been isolated. His main observations are on changes in RNA and RNAase in the brain; as he says, correlations of the kind he reports do not yet lead to any firm conclusions. Ann and Bruce Welch ("Isolation, Reactivity and Aggression: Evidence for an Involvement of Brain Catecholamines and Serotonin") also use isolated mice: their concern is with the metabolism, especially in the brain and adrenals, of three "biogenic" amines—noradrenaline, dopamine and serotonin. These substances are less rapidly metabolized by mice that have been kept alone. Once again, the authors are faced with the problem of interpreting correlations. The stage of testing predictions has not yet been reached.

R. Plotnik, D. Mir and J. M. R. Delgado give an account of "Aggression, Noxiousness, and Brain Stimulation in Unrestrained Rhesus Monkeys". By "aggression" they mean an approach to another monkey, a posture, a grimace, or a chase plus biting. Remarkable equipment enables them to evoke these responses, in socially appropriate circumstances, by stimulation of the brain from a distance. P. G. Bourne describes "Altered Adrenal Function in Two Combat Situations in Viet Nam". It was expected that the "stress" of flying would raise the 17-hydroxy-corticosteroid level in the urine, but this was not found. Bourne writes: "it appears that each subject utilizes very extensive and effective psychological defenses to handle the events with which he is faced. . . . For many reasons, warfare among nations cannot be equated either psychologically or physiologically with aggressive behavior in animals". This sober, critical comment deserves more attention than, probably, it will receive.

A feature of this volume is that most authors attempt some critical scrutiny of the concepts they use and the conclusions they draw. K. E. Moyer provides "A Preliminary Physiological Model of Aggressive Behavior". He emphasizes the diversity of the phenomena called "aggression", and lists eight "kinds of aggression", from predation to maternal defence of young. He might have gone further. One of his principal themes is the existence of "innately organized neural circuits for the various kinds of aggression". But he does not analyse the dubious concept of "innateness". Indeed, throughout the book, there is the usual confusion between genetical determination (of differences between individuals) and stability in development (of particular characters). Moyer's argument could reasonably have led to two conclusions: first, that each of his eight categories requires separate study and a separate name; second, that the ontology of predation, and of the

various kinds of social intolerance, must also be separately studied. This is implied by J. P. Scott, in an introductory paper on "Theoretical Issues"; but Moyer does not drive the point home. Similarly, Eleftheriou describes differences in the metabolism of biogenic amines related to different treatments and different behaviour; but he continues to call all the behaviour "aggressive". In two other chapters, in spite of a warning by Scott, even predation and its effects are discussed as if they were social interactions. There may be an underlying physiology common to, and also specific to, all this behaviour, but it has not yet been identified.

Another omission is on the biological and physiological nature of "isolation-induced aggression". As Scott says, this behaviour could be an effect of the novelty or strangeness of encounter with another animal. But, instead of analysing this odd phenomenon, experimenters call it "aggression" and use it as a behavioural index. There are still more important questions. How specific are the hormonal and other changes described? Wild rats under repeated attack develop enlarged, hyperactive adrenals; but the same applies to the attackers. Evidently, in this species, the adrenal cortical response is non-specific. What contribution, then, does all this endocrine activity make to survival? Despite these questions, often unasked, *The Physiology of Aggression and Defeat* will help all workers in this area. The authors and editors are to be congratulated on their rigorous experimental studies in a difficult field.

The aim of *The Imperial Animal* is "to describe what is known about the evolution of human behaviour, and then to try to show how the consequences of this evolution affect our behavior today". Topics include political systems, the status of women, property relationships, education, health and, of course, "aggression". There is a large bibliography. The authors' method is to make provocative assertions, often based on remote analogy, but unsupported by exact references. Statements on, say, the behaviour of baboons, which seem to contradict published findings, cannot be checked because they are not adequately documented. Sometimes, a list of sources is given, bearing on the topic discussed but not supporting the statement made. The authors assail the "culturalist", who "assumes that the human infant is a *tabula rasa* . . . unencumbered with the imperative, instinctive demands that manipulate animals" and with "appealing delusions about human perfectability". But they fail to identify people who hold these odd views; their comments on them neglect the need for definition of terms such as "instinctive"; and their arguments contain little evi-

dence but much dogmatic assertion.

Extensive reading enables Tiger and Fox to present a large, heterogeneous assembly of facts and notions; many, taken individually, are stimulating or entertaining. Their tone is agreeably humane. But sometimes they seem almost to be caricaturing another, better-known popular writer. In a lively passage on incest we read: "one enhancing spice of some sexual relationships seems to be a touch of that ritual neck-bite dominance the hamadryas baboon or the ambitious sultan appears to show". They also write: "aggression in the human species is the same as aggression in any other animal species". It is depressing to find such indifference to obvious problems, and to the need for rigour, in two academic anthropologists. This book falls in the domain of neither the natural nor the social sciences, but rather in the category of *belles-lettres*. S. A. BARNETT

Micropalaeontology

Micropalaeontology of Oceans. Edited by B. M. Funnell and W. R. Riedel. (Proceedings of the Symposium held in Cambridge from 10 to 17 September 1967 under the title "Micropalaeontology of Marine Bottom Sediments".) Pp. x+828. (Cambridge University: London, June 1971.) £20; \$55.

A book costing £20, even if written by many specialists and purporting to "become the definite reference compendium for students and workers in oceanography and palaeontology for many years to come", must be very exceptional to receive unqualified approval. By that criticism alone this useful volume is difficult to recommend.

This compendium, presented by more than forty separate authors and appearing four years after the conference at which the papers were read, must have produced headaches for the editors.

The general field covered is a rapidly expanding one and the time lag in publishing must make a number of the papers outdated. As is to be expected, a wide field of subjects is covered, but unfortunately this leads to the book being unbalanced in its emphasis of various subjects.

Some contributions are abstracts or little more, whereas others are long, containing much information which can now be considered of historical interest only. The printing of the maps and diagrams is often blurred and some of the photographic plates fall far below the high standard of recent years. Some papers, for example, "The systematics of coccoliths in relation to the palaeontological record" (M. Black) and "Systematic classification of polycystine Radiolaria" (W. R. Riedel), although important contributions to science have little or no bearing on the title of

the volume. Several consist of accounts of old geological "chestnuts", for example, "The direction of coiling in planktonic Foraminifera" (H. Bolli), and others of the "academic" boundary problems "Quaternary boundaries and correlations" (Hays and Berggren) and "Tertiary boundaries and correlations" (Berggren). Some of the correlation tables (for example 52.38) are in such a small scale a magnifying glass is necessary to interpret them.

Section A, "Distribution of living organisms in the oceans", sadly lacks comprehensive papers on both benthonic Foraminifera and Ostracoda. Here the "Distribution of ostracods in the oceans" (Puri) is inadequate, based not on recent work, but on the Challenger (1872-1876) collecting, apart from Puri's later work on the Bay of Naples. Considering the large group of workers on the ecology and distribution of ostracods, this gap should have been filled, the same being true of benthonic Foraminifera.

In section B there is a wide coverage of microfossil remains, but again, considering their contribution to bottom sediments (reefal and shelf sediments and so on), benthonic Foraminifera should have been included. Sections C and D again concentrate largely on planktonic organisms. The earlier papers "Distribution of shell-bearing pteropods in the oceans" (Chin Chen, in abstract only) and "The occurrence of Pre-Quaternary pteropods" (D. Curry) add little to our knowledge. Section E deals with the "boundary problems" which I mentioned earlier.

This book contains many ideas, some of which are new and worthy of further work, but could have been vastly improved by better illustrations, clearer maps and range charts and by better coverage of the general field. Some omissions could have been filled—the specialists exist who could do it.

T. BARNARD

Molecular Shapes

Conformational Analysis: Scope and Present Limitations. Edited by G. Chiurdoglu. (Papers presented at the Brussels International Symposium, September 1969.) (Organic Chemistry: a Series of Monographs, Vol. 21.) Pp. xi+280. (Academic: New York and London, January 1971.) \$15; £7.

CURRENT interest in stereochemistry and conformational analysis is wide-ranging and the importance of this area of chemistry is such that this record of the papers presented at the International Symposium on Conformational Analysis held in Brussels in September 1969, will certainly have a very wide appeal.

The conformational behaviour of five- (Altona), six- (Anteunis; Garbisch, Hawkins, and MacKay; Lyle, Thomas,

and Walsh; Reisse; Stollow), seven- (von Bredow, Friebolin, and Kabuss), and eight- (Anet) membered carbocyclic and heterocyclic systems is discussed theoretically and in relation to new experimental results. Timely reviews of other important topics are also very welcome. These include *ab initio* conformational analysis (Lehn), the examination of competitive pathways in conformational analysis (McKenna), stereochemical aspects of amine quaternization (Fodor, Mandava, Frehel, and Cooper), solvolysis reactions (Felkin), hypervalent molecules (Musher), carbonium ions (Schleyer), steroids (Bucourt), cyclodepsipeptides (Ivanov and Ovchinnikov), carbohydrates (Onodera), paraffins (Pucci, Aglietto, Luisi, and Pino), and catenanes (Schill).

This brief survey does not do justice to the scholarly character of most of these papers. The monograph is an important contribution to the chemical literature.

W. D. OLLIS

For Want of 0.016d

The Price of Amenity: Five Studies in Conservation and Government. By Roy Gregory. Pp. xvi+319. (Macmillan: London, 1971.) £8.00.

THIS is an important book for two reasons. First, it is a clear and balanced account of the ontogeny of decision-making in five cases of conflict between "developers" and "conservationists". On one side are arrayed a firm wanting to mine iron ore in Oxfordshire, the Central Electricity Generating Board wanting to put a generating station in the Nottingham green belt, a river authority wanting to build a reservoir in upper Teesdale, the Gas Council wanting to deliver North Sea gas on to the Norfolk coast, and the Southern Gas Board wanting to plant a tall gasometer in Abingdon. On the other side are arrayed the general public, preservation societies, naturalists, and local authorities, defending their sites against the encroachment of tech-

nological society. In each of the five cases the decision-making process began with rational argument based on careful calculations. But it soon became evident that feelings would carry more weight than facts in the final round of decision-making. The confrontation between the exploiter and those who resist him is analysed with refreshing lucidity. The issues are easy to describe and very difficult to resolve. They are: (i) How much are we prepared to pay for amenity (to keep mining out of Oxfordshire, to protect the flora of upper Teesdale, to preserve the skyline of Abingdon)? (ii) How do we calculate what amenity is worth in pecuniary terms? (iii) Who are "we"? The government, or the local authority, or the man-in-the-street?

Mr Gregory demonstrates that the answers to these questions cannot be given in advance of the decision-making. After the decision has been made it is sometimes possible to say how much it has cost to save a site from the developers. Thus it was asserted that to put a generating station at Holme Pierrepont, near Nottingham, would save £600,000 a year compared with running costs at other sites; equivalent to a change of 0.016 of a pre-decimal penny per unit of electricity. In the event the conservationists won: the generating station was built elsewhere. This means that in those circumstances, "we" were prepared to pay this higher price for electricity in order to preserve the strip of green belt near Nottingham. Also, when the conservationists lose, as they did at Cow Green in upper Teesdale, it is possible to quote a value (namely the extra cost of putting a reservoir on some other site on the Tees) which "we" were not prepared to pay for saving the scenic and botanical amenities of Cow Green. And, in recent years, "we" have grown from a small lobby of people who cherished the spirit of Blake and Cobbett to a massive public opinion determined to preserve the British countryside.

Mr Gregory's account is admirably fair and it should dispel the common misconception that the developer is always a philistine and the conservationist an altruist. Those who undertake massive industrial development, especially if they are public corporations providing electricity, water, or gas, owe it to the public to optimize their costs. But one of the inputs they include in their calculations is an estimate of what it will cost to choose the less beautiful, but also less suitable, site. Confrontation occurs when the developer believes that the estimate for choosing the less suitable site is too high, and the public, who may not have to bear the higher cost, challenge this belief. The public are, however, at a disadvantage in these confrontations, because they do not have access to the data on which the developer's calculations are made; and they are evaluating amenity.

There is no doubt that case histories of the kind Mr Gregory presents in this book are the best way to illuminate those very important processes of decision-making which begin with masses of technological data and cost-benefit analyses and end with what one might call "informed hunches". This is the second reason why Mr Gregory's book is important. It is a model of the way to teach students the art of technological decision-making. In Alvin Weinberg's words, it is a casebook for "mission-oriented" as contrasted with "discipline-oriented" study. Many students, both arts-based and science-based, will be involved in this sort of decision-making all their lives, in industry or in public corporations or in government. At present they find little in their undergraduate courses which prepares them for this style of thinking. They, and their teachers, should be encouraged to read this book. It is a pity that its price is so unreasonably high. If this is due to the maps, the publishers should be encouraged to bring it out as a paperback without maps.

ERIC ASHBY

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